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MDCCCXXVIII.

PHILOSOPHICAL

THE ASSAULT

BY J. M. W. TURNER

THE ASSAULT, OR THE BARRICADE, WAS THE LAST OF A SERIES OF PICTURES BY TURNER ON THE SUBJECT OF THE PENINSULAR WAR.

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Presents received by the Royal Society, from 17th November 1827, to 19th June 1828.

Meteorological Journal, kept at the Apartments of the Royal Society, by order of the President and Council.

PHILOSOPHICAL TRANSACTIONS.

XII. *A description of a vertical floating collimator ; and an account of its application to astronomical observations with a circle and with a zenith telescope.*
By Captain HENRY KATER, V.P.R.S.

Read April 24, and May 1, 1828.

IN the Philosophical Transactions for 1825, I gave an account of a floating collimator, and added a suggestion for the construction of a vertical floating collimator which had not then been carried into effect. I have since had an instrument of that description made, with such improvements as occasion required, and the results which it has afforded have been so satisfactory that I am induced to lay them before the Society.

The collimator which formed the subject of the paper I have mentioned was a *horizontal* floating collimator. This, in the manner in which I then used it, was the worst form in which the instrument could have been employed ; as it was necessary to take the float out of the mercury and replace it in order to complete each observation. The result was therefore liable to be vitiated by any particle of dust or minute bubble of air which might have found a place between the float and the mercury. It cannot therefore but be considered as surprising, that out of one hundred and fifty-one results, only twenty-eight were found in error to an amount exceeding one second, the greatest error being 2".58 and the next 2".

The horizontal floating collimator was tried by the Rev. Dr. BRINKLEY, the present Bishop of Cloyne at the Dublin Observatory, and by the Rev. Dr. ROBINSON at the Observatory at Armagh. An account of Dr. BRINKLEY's observations is given in the Philosophical Transactions for 1826, where it may be

seen that the mean difference between the results of Dr. BRINKLEY's catalogue of 1823, and those obtained by means of the horizontal floating collimator by observations upon twenty stars, is only $0''.03$.

Dr. ROBINSON, by ten observations of five stars made with the horizontal floating collimator and his equatorial, obtained a latitude differing only $0''.02$ from the latitude resulting from his observations of the preceding two years, when the level was employed; and by thirteen observations with the collimator at the winter solstice of 1825, the deduced obliquity of the ecliptic for the beginning of the year, differed only $0''.33$ from that given in the Nautical Almanac.

These results should seem to leave little to be desired in point of accuracy; but the method of using the horizontal floating collimator is so inconvenient, as to constitute no small objection to the general employment of the instrument in this form. To which may be added, the possibility of error arising, as before stated, from the necessity of taking the float out of the mercury and replacing it. From both these objections the vertical floating collimator is wholly free.

The vertical floating collimator has also this further advantage, that it may not only be used with a circle, but may be applied to a telescope, either of the refracting or reflecting kind; such a telescope furnished with a wire micrometer and directed to the zenith, becomes a zenith telescope, free from all the objections to which the zenith sector and the zenith telescope with a plumb-line are liable.

In Plate XIII. I have given plans and sections of the different parts of which the vertical floating collimator is composed. Fig. 1. represents a board of well seasoned mahogany fourteen inches square, and an inch and a half thick. Into this board four legs are screwed, at the distance of an inch and a half from the edge of the board to the centre of each leg. In the middle of the board a circular hole is made, four inches in diameter, into which a tube of sheet iron is firmly driven, of such a length as to project about an inch or an inch and a half above the upper surface of the board. At the distance of five inches and a half from the centre, three brass rollers are let into the board. These are equidistant from each other, and are intended to support the iron pan hereafter to be described, and to facilitate its being moved round about the sheet iron tube as a centre, with but little friction.

Fig. 1.

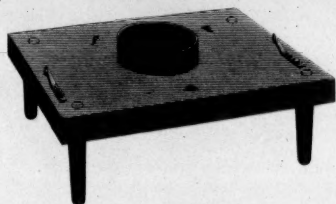


Fig. 2.

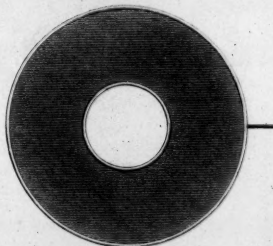


Fig. 4.



Fig. 5.

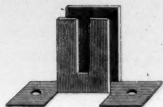


Fig. 3.

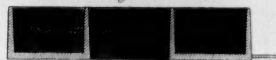


Fig. 6.



Fig. 7.

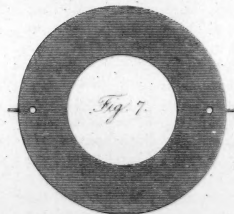


Fig. 10.

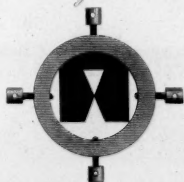


Fig. 11.

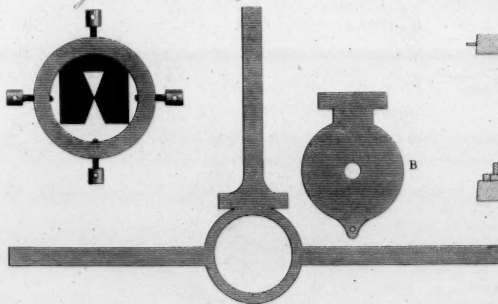
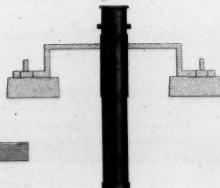
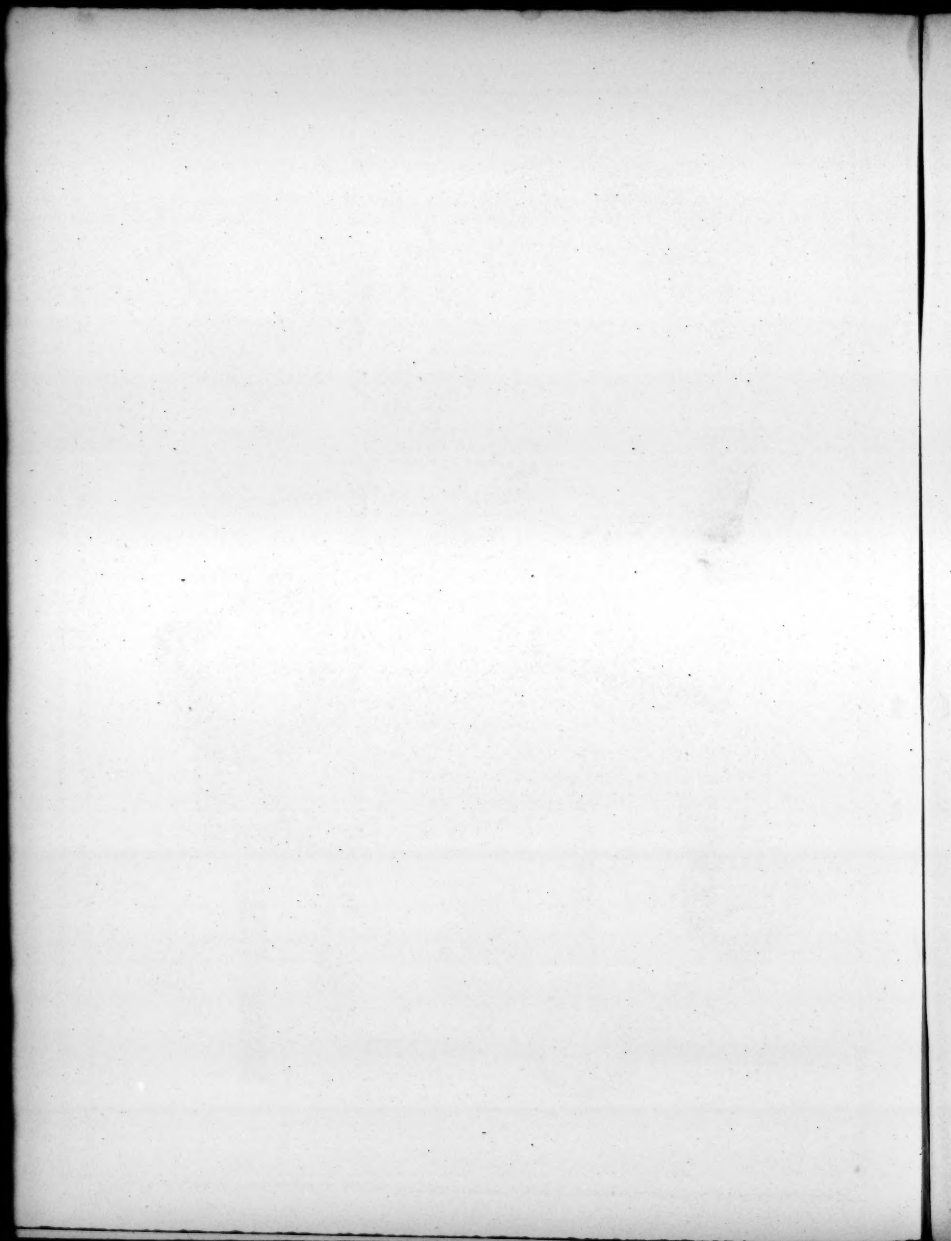


Fig. 8.



Fig. 9.





Figs. 2. and 3. are the plan and section of a cast iron circular pan ; this is one foot in diameter, and has a circular opening of four inches, which passes easily upon the tube of sheet iron just described, and about which the pan is intended to be turned. The sides of the pan (fig. 3.) are 2.4 inches high on the outside, the inside depth being two inches, which leaves four-tenths of an inch for the thickness of the bottom of the pan. The sides of the model for the pan must be sloped as in the section, for the convenience of casting. Into the side of the pan at the bottom a stout iron wire is screwed, intended, as will be seen, to serve as a stop to prevent the pan from being turned round more than 180 degrees.

Figs. 4. and 5. represent (in half its real dimensions) one of two guides made of sheet iron, destined to receive pins, which are intended to prevent the float, presently to be described, from moving horizontally. A piece of plate iron is cut into the form represented at fig. 4. This is afterwards turned up as at fig. 5. leaving the distance of a quarter of an inch between the front and back. Two of these guides are screwed to the bottom of the pan in the inside, (their backs touching its sides,) in that diameter which is at right angles to the iron pin projecting from the outside of the pan.

Fig. 6. represents a spring of about two inches and a half long. The flat part has a longitudinal slit in it, through which screws pass which attach it to the mahogany board, fig. 1. A piece of plate brass is interposed between the screws and the spring. The longitudinal slit is for the purpose of adjusting the spring to its required position. At its extremity the spring is formed into a Y, terminating in a hook, as represented in the figure. Two of these springs are fixed to the mahogany board ; the Y of each being distant a little more than six inches from the centre of the board, in opposite directions, and the direction of the spring is at right angles to this diameter. The springs both point the same way.

If now we suppose the pan to be placed upon the sheet iron tube, fig. 1. and it be turned round, the iron pin will come in contact with one of the springs, which will yield to the weight of the pan until the pin is lodged in the Y, beyond which it will be prevented from going by the projecting hook. On turning the pan the contrary way, the inclined plane of the Y will yield to the pressure of the pin and permit it to escape ; and when the pin has gone through

nearly a semicircle, the same process will take place at the other spring; these springs, therefore, afford the means of limiting the horizontal motion of the pan to 180 degrees.

Figs. 7. and 8. are the plan and section of a float of cast iron, 10.6 inches diameter, one inch thick, and having an opening in the middle of 5.7 inches diameter, consequently the breadth of the annulus is about two inches and a half. Into the sides of this float, at opposite points and equidistant from the two surfaces, two steel pins are screwed, of such a thickness as to pass freely into the grooves of the guides before mentioned with a very little shake. The ends of the pins are to be hemispherically rounded, and very smooth. When the float is placed in the pan, with the pins in the guides, the distance between the terminations of the pins should be such as to leave them just clear of the backs of the guides.

A section of the float with the bridge and telescope attached to it is given at fig. 9. The bridge is made of wrought iron. The length at the top is seven inches, and the perpendicular part of the bridge is of a sufficient height to enable the bridge to clear the middle part of the pan when the float is placed in it. In my vertical floating collimator the bridge is an inch and three-quarters above the float. The middle of the bridge consists of a piece of brass tube three or four inches long, destined to receive the telescope, which fits tightly into it. The width of the iron part of the bridge is half an inch, and the thickness about a quarter of an inch; but this may be varied according to circumstances which will hereafter appear. The parts where the bridge is screwed to the float have cross pieces four inches long, intended to give a firmer bearing. The heads of the screws are long, to serve as pins, upon which weights with holes in them may be placed, for the purpose of adjusting the float.

The bridge is screwed to the float in such a position that its length is at right angles to the pins inserted in the edge of the float, as before described. The intention being, that when the collimator is employed, the bridge should be in the direction of the meridian, and the guides at right angles to it. Two additional pins are fixed perpendicularly in the float near the guide pins. These are represented in the plan, fig. 7, and are intended to receive some of the weights by which the float is to be adjusted.

The telescope is achromatic, the object-glass in my collimator being about

eight inches focal length, and one inch and a quarter aperture. The object-glass is fixed in a separate piece of tube, which slides within another tube, to which, after adjustment, it may be firmly attached by two opposite screws moving in longitudinal slits.

In the focus of the object-glass of my collimator, a diaphragm is placed, carrying fine cross wires flattened. These wires, however, do not form angles so neat as could be wished, in consequence of their thickness, and of a want of perfect straightness of their edges; and I am indebted to Dr. WOLLASTON for the suggestion of a method of constructing a substitute for the cross wires, which has been applied to a vertical floating collimator made for Captain FOSTER, R.N. and which I found to answer perfectly well.

The surfaces of a plate of brass (bell metal would perhaps be preferable), about the twentieth of an inch thick, were ground parallel. The plate was then cut in half, and the surfaces cemented together. One of the sides of this double plate was then formed by grinding, into a salient angle of about 135° , the faces of this angle being slightly bevilled and carefully finished. The plates were then separated, and their opposite surfaces cemented together, the angular points and edges being made accurately to coincide. One of the sides (not that opposite to the angle) of this compound plate was then ground at right angles to the surface, and the plates again separated and carefully cleaned.

A circular piece of glass having been fitted into the diaphragm, a bit of brass with a straight edge was cemented to the glass on one side by shell lac. This was at such a distance from the middle of the diaphragm, as to allow the angular points of the brass plates to be in its centre, when their ground sides were in contact with the straight edge.

One of the brass plates was now attached by shell lac to the glass, having its ground edge in contact with the brass straight edge, the obtuse angle of the bevilled edge being next the glass, and the angular point in the middle of the diaphragm. It is now evident that if the ground edge of the other brass plate be placed in contact with the straight piece of brass, the obtuse angle of the bevil being next the glass, the two angular points will, upon sliding the brass plate along the straight edge, be brought accurately into contact, and in this position the brass plate was fixed to the glass by shell lac.

This arrangement afforded an object having well defined acute angles of

about 45° , which could be bisected with great precision. As this is a very important part of the instrument, I have been induced to describe thus minutely the manner in which it may be made. The diaphragm is placed in the telescope, with the brass plates next to the object-glass, in order that when the edges of the plates are seen with perfect distinctness, no particles of dust may be visible which may have lodged upon the surface of the glass. The angular point is brought into the centre of the tube in the usual manner, by means of opposite screws passing into the diaphragm. A piece of plane glass closes the end of the telescope, to keep out dust and to protect the diaphragm from accident. The diaphragm is represented at fig. 10*.

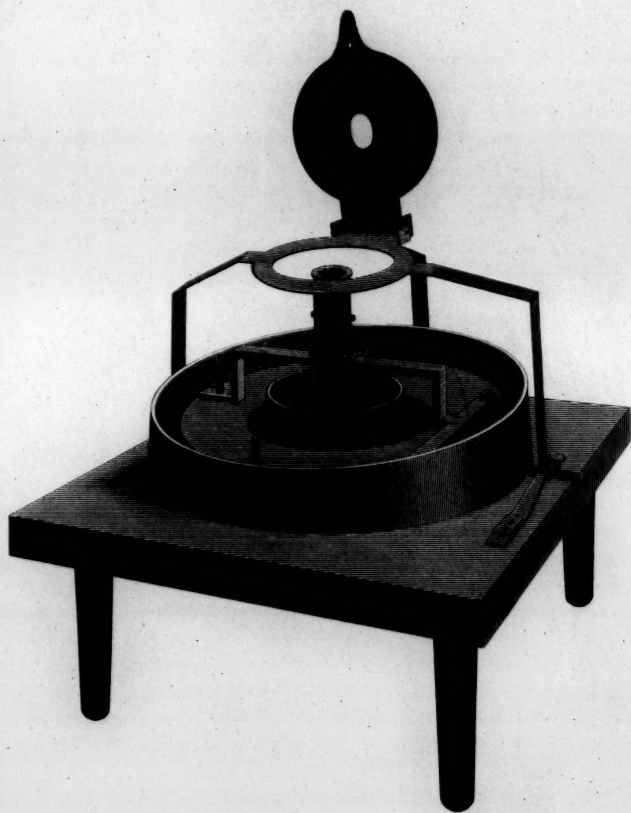
When observations are made with the floating collimator, it is necessary that all light should be excluded, except that which passes through its telescope. For this purpose a piece of sheet iron is formed, as represented at fig. 11. The aperture is the same as that of the iron basin of the collimator, and one side externally is straight, in order to form a hinge upon which the plate B moves. This plate may be raised to a perpendicular position, and when permitted to fall, rests steadily upon the ring. In the centre of the plate B, is a circular opening, rather less in diameter than the telescope. The three projecting parts of fig. 11. are intended, when bent close to the ring, to form legs, to be attached to the mahogany support, fig. 1, the length of these legs being such as to allow the plate B, when resting on the ring, to be quite clear of the telescope of the collimator.

For the purpose of illuminating the diaphragm of the collimator, a small plane mirror is used, similar to that of a microscope. This is attached to the plate B over the opening, by a short firm pedestal, the manner of doing which is so obvious, as to need no figure. The instrument is represented in Plate XIV.

The collimator being thus completed, it will be found convenient to adjust the object-glass of its telescope very nearly to the distance of its focal length from the diaphragm. For this purpose, any telescope may be employed which has been carefully adjusted to distinct vision upon a fixed star. The telescope of the collimator is to be supported in a convenient position for looking into it through its object-glass with the other telescope, and a lamp so placed as to illuminate the diaphragm. All false light being excluded from the telescope

* Since the above was written, I have found that a cross of strong spiders threads answers very well.





employed, by a black paper screen surrounding that of the collimator, the diaphragm is to be viewed, and the distance of the object-glass from it to be varied until its angles appear sharp and well-defined. This I consider merely as preparatory to the final adjustment; but if the telescope used has a magnifying power of about one hundred, this adjustment will probably be found to be sufficiently accurate.

Two beams of wood are to be fixed horizontally to the walls of the observatory in the direction of the meridian, beneath the opening in the roof, and at such a distance from each other as that the legs of the support of the collimator may pass between them, and the board rest upon the beams. It may then be moved along upon the beams, the legs confining the motion in the direction of the meridian. In order to render this motion more free, and to prevent the beams from wearing, their upper surfaces are covered with sheet iron, and four rollers are let into the board near the legs, one of the rollers being attached to a spring in order that all four may be always in contact with the beams.

The instrument employed for the final adjustment of the collimator and in the zenith observations which will hereafter be detailed, was a reflecting telescope of the Newtonian construction, having an aperture of six inches and a quarter, a focal length of forty inches, and a magnifying power of ninety-nine times. This was furnished with a spider's thread micrometer, each of the divisions of which is equal to half a second.

A slight frame of wood was made to receive the tube of the telescope when placed in the direction of the zenith. This frame consisted of a triangle embracing the tube a little below the micrometer. From the corners of this triangle, three rods of wood pass into the extremities of a T made of wood, the cross piece being about two feet in length. Near the extremities of the T, are also inserted coarse iron screws, for the purpose of placing the telescope in the direction of the zenith. The tube of the telescope is kept steady in the triangle by two small wedges of wood, the end containing the mirror resting on the middle of the perpendicular bar of the T. The telescope previously to its being placed in the observatory, was very nicely adjusted to distinct vision upon a fixed star*.

* I have stated rather what ought to have been done, than what was actually effected at the time, as will be seen hereafter.

The room in which the observations were made, has a pit in it about two feet and a half wide, six feet long, and seven feet deep, filled with sand, upon which are laid two flat stones clear of the sides of the pit. These form a support for any instrument I employ, which is thus secured from any perceptible tremor which might be occasioned by passing carriages.

The Newtonian telescope, in its wooden frame, is placed under the collimator in such a position, that a plumb-line let fall from the centre of the collimator, passes on one side of the plane mirror of the telescope. The micrometer in my telescope is on the east side of the meridian, the micrometer head being to the right-hand of the observer; consequently the moveable line is perpendicular to the horizon, and from the nature of the telescope parallel to the equator. This line should be nicely adjusted so as to represent a parallel of declination, by turning the micrometer round in the eye-tube until a fixed star (γ Draconis in my observatory) runs along the wire.

A sufficient quantity of mercury is put into the basin of the collimator to support the float without risk of its touching the bottom. The diaphragm is illuminated by a lanthorn furnished with a lens, and placed upon a support attached to the board of the collimator. The illumination of the diaphragm was attended with much trouble in my first trials, but I afterwards discovered a method by which the difficulty was obviated. The end of the Newtonian telescope being closed by its cover, a sheet of white paper was placed upon it; the light of the lanthorn was thrown upon the plane mirror, and the position of the mirror varied until the shadow of the diaphragm appeared upon the paper.

If the diaphragm should not now be visible in the telescope, the telescope must be inclined to one side or the other by means of the foot-screws, till the diaphragm is seen in the centre of the field of view.

The next step is to place the line of collimation of the telescope of the collimator nearly in the direction of the zenith. For this purpose, the telescope is to be adjusted by its foot-screws, so that the horizontal thread of the micrometer shall pass through the angular point of the diaphragm, when the basin of the collimator must be turned half round. It is now possible that the diaphragm may be no longer in the field of view; in which case, whilst an assistant looks through the telescope, the float of the collimator must be depressed by the finger on one side or the other, until the diaphragm is again seen just

within the field. Weights must then be put upon the pins to which the pressure was applied, and the process repeated till the centre of the diaphragm remains in the middle of the field of view, upon the collimator being turned half round. The telescope must of course be made to follow these adjustments by means of its foot-screws. The telescope of the collimator must now be turned round in its tube, (the pin on the outside of the basin being in one of the Y's,) until the diaphragm is brought into that position in which both its angles can be bisected by the moveable line of the micrometer. When the adjustment is sufficiently accurate, the angular point of the diaphragm will remain upon the horizontal line, or very near it, and not depart much from the moveable line of the micrometer when the collimator is turned half round.

The collimator is now to be moved along the cross beams until the angles become faintly illuminated, when they are to be carefully bisected with the moveable line. The collimator is then to be slid the contrary way till the angles again become faint, when, if the bisection is not as perfect as before, the distance of the object-glass of the collimator from the diaphragm is not adjusted to the last degree of precision. If the diaphragm appears to have moved from the line in the opposite direction to that in which the collimator was moved, the rays diverge, and the object-glass is too near the diaphragm. If the diaphragm has moved in the same direction as the collimator, the rays converge, and the object-glass must be brought nearer to the diaphragm. When the angles continue perfectly bisected in both positions of the collimator, the object-glass is to be secured in its place by means of the screws for that purpose.

We have seen that when the iron pin was in one of the Y's, the telescope of the collimator was turned round in the tube which carries it, till the angles of the diaphragm could be bisected by the moveable line of the micrometer. But it is possible that upon lodging the iron pin in the other Y, the angles may no longer be capable of bisection. This would happen if the basin of the collimator had not described a semicircle; and in that case, the spring or Y must be shifted till the angles can be perfectly bisected, when it must be firmly fixed in its place*.

* It is evident that the adjustments which have been, I trust with sufficient minuteness, described, may be effected by employing the telescope attached to an astronomical circle, instead of using a detached telescope for the purpose.

I shall now show the manner in which the vertical floating collimator is to be used for determining the zenith point of an instrument, and the degree of dependence that may be placed upon the results.

The collimator is to be turned half round, and the iron pin lodged in one of the Y's. This is done merely to agitate the float slightly, in order to release the parts from any possible constraint. When the float is quite steady, which it will be in about half a minute, the angles of the diaphragm are to be carefully bisected by the moveable thread of the micrometer, and the division read off and registered. I have generally bisected the opposite angles separately, as will be seen in the tables which follow. The collimator is now to be turned half round, the angles again bisected, and the division of the micrometer read off. The mean of these readings will be the zenith point of the instrument employed, and half their difference will be the angle formed by the line of collimation of the telescope of the collimator with the zenith.

It must be evident that the accuracy of the results afforded by the floating collimator (excepting the unavoidable errors of observation) is wholly dependent upon the permanency of its line of collimation to the zenith, during the very short interval between the two observations, or bisections of the angles of the diaphragm which have been described. But this inclination may vary considerably from expansion, or otherwise, between any two determinations of the zenith point, without at all influencing the accuracy of either.

The agreement of the several determinations of the zenith point with each other, will depend upon the degree of stability with which the astronomical circle, or the zenith telescope is supported, and upon the supposition that its parts suffer no relative change of position from unequal temperature or otherwise. The time required for completing the determination of the zenith point by means of the vertical floating collimator, does not exceed two minutes; and if to this be added the time necessary for another determination of the zenith point, the whole time required will not be more than five minutes, during which, it may be allowable to suppose that no very sensible change can take place, either in the collimator or in the instrument, from variation of temperature. We will therefore consider the mean of these two separate determinations as the true zenith point, and conclude the difference between this mean, and the first determination of the zenith point to be the probable error with which such first determination can be charged. The second determination of

the zenith point will have the same error as the first, but with a contrary sign. This second determination, however, will also have another error deduced by taking the difference between it, and the mean of the second and third readings of the zenith point. The mean of these two errors is considered to be the probable error of the second zenith point, and so of the rest. It would not be safe to take the mean of all the determinations of the zenith point which constitute a series for the standard of comparison, as the instrument may, and probably will, have suffered some slight change of position during so long a period. If the errors (supposed to belong to the floating collimator) be applied to the observed zenith points, the results will show the whole change of position which the zenith telescope may have suffered during the observations constituting the series; and as it may be interesting to see the degree of stability of an instrument supported in the slight manner I have described, I have added a column of the corrected zenith points.

I shall now proceed to detail the experiments that have been made; and in order that the inferences may not rest solely upon my own authority, I shall give the observations of such gentlemen as have been from time to time kind enough to assist me.

The Collimator turned half round rapidly and without care.								
Date. 1827.	Reading of the Micrometer. Divisions.	Mean.	Inclination of the Collimator. Divisions.	Inclination in Seconds.	Reading at the Zenith. Divisions.	Zenith Point in Seconds.	Possible Error in Seconds.	Corrected Zenith Point.
Oct. 20.	79.5	78.40	2.57	1.28	75.82	37.91	+ 0.10	37.81
	77.3							
	71.7							
	74.8	77.35	1.92	0.96	75.42	37.71	+ 0.00	37.71
	79.0							
	75.7							
	72.8	73.50	2.40	1.20	75.00	37.50	- 0.11	37.61
	74.2							
	78.8							
	76.0							
	73.0							
	72.2							
						Mean	0.00	

The Collimator turned half round rapidly and without care.								
Date. 1827.	Reading of the Micrometer. Divisions.	Mean.	Inclination of the Collimator. Divisions.	Inclination in Seconds.	Reading at the Zenith. Divisions.	Zenith Point in Seconds.	Possible Error in Seconds.	Corrected Zenith Point.
Oct. 21.	63.0	63.00	} 11.17	5.58	74.17	37.08	-0.11	37.19
	63.0							
	83.0							
	87.7	85.35	} 10.40	5.20	74.60	37.30	+0.05	37.25
	64.2							
	64.2							
	82.0	85.00	} 9.67	4.83	74.67	37.33	-0.02	37.35
	88.0							
	65.0							
	65.0	84.35	} 8.92	4.46	74.92	37.46	+0.01	37.45
	82.0							
	86.7							
	66.0	83.85	} 10.07	5.03	74.57	37.28	-0.10	37.38
	66.0							
	81.7							
	86.0	84.50	} 9.00	4.50	75.0	37.50	+0.08	37.42
	64.5							
	64.5							
	81.0	84.65	} 8.95	4.47	73.95	36.97	0.00	36.97
	88.3							
	66.0							
	66.0	84.00	} 8.87	4.43	72.87	36.43	-0.18	36.61
	81.0							
	87.0							
	65.0	81.75	} 9.75	4.87	73.25	36.62	+0.08	36.54
	65.0							
	80.0							
	85.8	83.00	} 8.92	4.46	72.92	36.46	+0.07	36.39
	64.0							
	64.0							
	78.0	81.85	} 10.00	5.00	72.00	36.00	-0.13	36.13
	85.5							
	63.5							
	63.5	83.00	} 9.12	4.56	72.12	36.06	+0.02	36.04
	80.0							
	86.0							
	64.0	82.00	} 10.27	5.13	72.07	36.03	-0.02	36.05
	64.0							
	78.2							
	85.5	82.35	} 9.12	4.56	72.12	36.06	+0.02	36.04
	63.0							
	63.0							
	78.2	81.25	} 10.27	5.13	72.07	36.03	-0.02	36.05
	84.3							
	61.8							
	61.8	61.80	} 10.27	5.13	72.07	36.03	-0.02	36.05
	79.2							
	85.5							
						Mean . . .	-0.01	

A VERTICAL FLOATING COLLIMATOR.

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The Collimator turned half round slowly and carefully.

Date. 1827.	Reading of the Micrometer. Divisions.	Mean.	Inclination of the Collimator. Divisions.	Inclination in Seconds.	Reading at the Zenith. Divisions.	Zenith Point in Seconds.	Possible Error in Seconds.	Corrected Zenith Point.
Oct. 22.	7.0	7.00	} 18.75	9.37	25.75	12.87	-0.36	13.23
	7.0							
	42.0	44.50						
	47.0		} 19.20	9.60	27.20	13.60	+0.23	13.37
	8.0	8.00						
	8.0							
	43.8	46.40	} 18.80	9.40	26.80	13.40	-0.14	13.54
	49.0							
	8.0	8.00						
	43.0	45.60	} 19.52	9.76	27.52	13.76	+0.29	13.47
	48.2							
	8.0	8.00						
	43.8	47.05	} 18.90	9.45	25.90	12.95	-0.26	13.21
	50.3							
	7.0	7.00						
	42.3	44.80	} 19.15	9.57	26.35	13.17	+0.04	13.13
	47.3							
	7.2	7.20						
	42.7	45.50	} 19.47	9.73	26.47	13.23	+0.06	13.17
	48.5							
	7.0	7.00						
	44.2	45.95	} 19.90	9.95	26.10	13.05	0.00	13.05
	47.7							
	6.2	6.20						
	43.8	46.00	} 19.10	9.50	25.80	12.90	-0.08	12.98
	48.2							
	6.7	6.70						
	43.0	44.90						
	46.8							
						Mean . . .	-0.02	

Oct. 27.	88.2	89.65	} 6.87	3.43	82.77	41.38	-0.07	41.45
	91.1							
	78.0	75.90						
	73.8		} 5.90	2.95	83.05	41.52	-0.11	41.63
	86.2	88.95						
	91.7							
	78.0	77.15						
	76.3							

Date. 1827.	Reading of the Micrometer. Divisions.	Mean.	Inclination of the Collimator. Divisions.	Inclination in Seconds.	Reading at the Zenith. Divisions.	Zenith Point in Seconds.	Possible Error in Seconds.	Corrected Zenith Point.
Oct. 27. (Continued.)	88.0	90.55	} 6.32	3.16	84.22	42.11	+ 0.23	41.88
	93.1							
	79.0	77.90						
	76.8		} 5.45	2.72	83.50	41.75	+ 0.31	41.44
	85.4	88.95						
	92.5							
	78.6	78.05	} 4.77	2.38	81.77	40.88	+ 0.03	40.85
	77.5							
	84.1	86.55						
	89.0		} 4.62	2.31	79.77	39.88	- 0.28	40.16
	78.0	77.00						
	76.0							
	82.5	84.40	} 4.45	2.22	80.05	40.02	+ 0.05	39.97
	86.3							
	77.0	75.15						
	73.3		} 4.57	2.28	79.92	39.96	- 0.08	40.04
	82.0	84.50						
	87.0							
	76.8	75.60	} 4.60	2.30	80.50	40.25	+ 0.14	40.11
	74.4							
	82.0	84.50						
	87.0		} 4.57	2.28	79.92	39.96	- 0.08	40.04
	77.0	75.35						
	73.7							
	82.7	85.10	} 4.60	2.30	80.50	40.25	+ 0.14	40.11
	87.5							
	77.0	75.90						
	74.8							
						Mean ..	+ 0.02	

Oct. 28.	63.5	66.85	} 21.50	10.75	45.35	22.67	+ 0.21	22.46
	70.2							
	23.0	23.85						
	24.7		} 20.77	10.38	44.52	22.26	- 0.21	22.47
	60.6	65.30						
	70.0							
	23.0	23.75	} 19.97	9.98	45.37	22.68	+ 0.05	22.63
	24.5							
	62.0	65.35						
	68.7		} 18.97	9.48	45.77	22.88	+ 0.11	22.77
	24.5	25.40						
	26.3							
	60.8	64.75	} 18.00	9.00	45.35	22.67	- 0.11	22.78
	68.7							
	25.7	26.80						
	27.9		} 18.00	9.00	45.35	22.67	- 0.11	22.78
	60.7	63.35						
	66.0							
	26.2	27.35						
	28.5							
						Mean ..	+ 0.01	

Experiments by Mr. HERSCHEL.

Date. 1827.	Reading of the Micrometer. Divisions.	Mean.	Inclination of the Collimator. Divisions.	Inclination in Seconds.	Reading at the Zenith. Divisions.	Zenith Point in Seconds.	Possible Error in Seconds.	Corrected Zenith Point.
Nov. 26.	87.2	86.10	} 33.95	16.97	52.15	26.07	+ 0.37	25.70
	85.0							
	21.8	18.20	} 33.77	16.88	50.67	25.33	- 0.21	25.54
	14.6							
	85.9	84.45	} 33.90	16.95	50.90	25.45	+ 0.25	25.20
	83.0							
	20.3	16.90	} 34.97	17.48	49.17	24.58	- 0.09	24.67
	13.5							
	86.4	84.80	} 35.02	17.51	48.17	24.08	- 0.11	24.19
	83.2							
	21.4	17.00	} 34.30	17.15	48.10	24.05	- 0.02	24.07
	12.6							
	87.1	84.15	} 33.95	16.97	52.15	26.07	+ 0.37	25.70
	81.2							
	17.8	14.20	} 8.82	4.41	21.87	10.43	- 0.39	10.82
	10.6							
	84.2	83.20	} 9.27	4.68	22.42	11.21	+ 0.39	10.82
	82.2							
	16.2	13.15	} 8.82	4.41	21.87	10.43	- 0.39	10.82
	10.1							
	83.8	82.40	} 9.45	4.72	24.05	12.02	+ 0.34	11.68
	81.0							
	16.2	13.80	} 11.22	5.61	22.67	11.33	- 0.42	11.75
	11.4							
						Mean....	+ 0.03	

Experiments by Mr. BAILY.

1828.	17.0	13.15	} 9.27	4.68	22.42	11.21	+ 0.39	10.82
Jan. 20.	9.3							
	32.2	31.70						
	31.2							
	16.2	13.05	} 8.82	4.41	21.87	10.43	- 0.39	10.82
	9.9							
	31.9	30.70						
	29.5							

Experiments by Colonel COLBY.

Jan. 20.	36.0	34.60	}	9.90	4.95	24.7	12.35	+ 0.51	11.84	
	33.2									
	18.7									
	10.9	14.80	}	11.22	5.61	22.67	11.33	- 0.42	11.75	
	35.2									
	32.6									
	14.0	11.45	}	9.45	4.72	24.05	12.02	+ 0.34	11.68	
	8.9									
	34.9									
	32.1	33.50	}	9.45	4.72	24.05	12.02	+ 0.34	11.68	
	17.0									
	12.2									
								Mean. . .	+ 0.14	

CAPTAIN KATER'S DESCRIPTION OF

Experiments by Mr. South.

Date. 1828.	Reading of the Micrometer. Divisions.	Mean.	Inclination of the Collimator. Divisions.	Inclination in Seconds.	Reading at the Zenith. Divisions.	Zenith Point in Seconds.	Possible Error in Seconds.	Corrected Zenith Point.
Jan. 22.	25.3	24.00	} 19.10	9.55	43.10	21.55	+ 0.29	21.26
	22.7							
	64.1							
	60.3	62.20	} 18.52	9.26	41.97	20.98	+ 0.09	20.89
	24.6							
	22.3							
	60.5	60.50	} 18.50	9.25	40.10	20.05	- 0.51	20.56
	60.5							
	22.8							
	20.3	58.65	} 20.37	10.18	42.37	21.18	+ 0.48	20.70
	58.5							
	58.8							
	25.3	22.00	} 19.30	9.65	40.75	20.37	- 0.41	20.78
	18.7							
	63.8							
	61.7	21.45	} 18.60	9.30	40.75	20.37	- 0.41	20.78
	23.3							
	19.6							
	59.3	60.05	} 18.60	9.30	40.75	20.37	- 0.41	20.78
	60.8							
						Mean....	- 0.01	

Experiments by Captain SABINE.

Jan. 25.	69.0	66.90	} 19.70	9.85	47.2	23.60	+ 0.13	23.47
	64.8							
	30.0							
	25.0	27.50	} 19.50	9.75	46.7	23.35	- 0.19	23.54
	67.8							
	64.6							
	29.0	27.20	} 18.00	9.00	47.7	23.85	+ 0.32	23.53
	25.4							
	67.6							
	63.8	29.70	} 19.45	9.77	46.10	23.05	+ 0.03	23.02
	31.5							
	27.9							
	66.3	65.55	} 18.60	9.30	44.75	22.37	- 0.34	22.71
	64.8							
	28.0							
	25.3	26.65	} 18.60	9.30	44.75	22.37	- 0.34	22.71
	65.5							
	61.2							
	27.7	26.15	} 18.60	9.30	44.75	22.37	- 0.34	22.71
	24.6							
						Mean....	- 0.01	

For greater convenience, I shall now give in one view the errors with which each of the preceding determinations of the zenith point can be charged, classing together such as do not exceed one-tenth of a second; those between one and two tenths; those between two and three tenths; those between three and four tenths; those between four and five tenths; and such as exceed half a second.

TABLE of the Errors of the preceding determinations of the Zenith Point.

Not exceed- ing One Tenth of a Second.	Between One and Two Tenths.	Between Two and Three Tenths.	Between Three and Four Tenths.	Between Four and Five Tenths.	Above Five Tenths.
+ .10	— .11	+ .23	— .36	— .42	+ .51
.00	— .11	+ .29	+ .31	+ .48	— .51
+ .05	— .18	— .26	+ .37	— .41	
— .02	— .13	+ .23	+ .39		
+ .01	— .14	— .28	— .39		
— .10	— .11	+ .31	+ .34		
+ .08	+ .14	— .31	+ .32		
.00	+ .11	— .31	— .34		
+ .08	— .11	+ .35			
+ .07	— .11	+ .29			
+ .02	+ .13				
— .02	— .19				
+ .04					
+ .06					
.00					
— .08					
— .07					
+ .03					
+ .05					
— .08					
+ .05					
— .09					
— .02					
+ .09					
+ .03					

We may here perceive, that of sixty independent determinations of the zenith point, there are twenty-five, the error of each of which does not exceed one tenth of a second, thirty-seven under two tenths, forty-seven under three tenths, fifty-five under four tenths, three between four and five tenths, and two a little above half a second.

But it is probable that the greater part of these errors, minute as they are, must be attributed to want of power in the micrometer; for I found that when

the collimator was stationary, the repeated bisections of the same angle would generally differ one or two divisions from each other; and it must be remembered that one division is equal to half a second. The focal length of the telescope employed is only forty inches, and the power or scale of the micrometer, and consequently the precision of which it is capable, is directly as the focal length of the object-glass or mirror of the telescope to which it is attached.

The zenith telescope, and the telescope of the collimator, may be considered as forming together a compound microscope, having, as it were, a separable object-glass, the two parts of which may be placed at any distance from each other without altering the effect. The magnifying power of this microscope may be found in the usual manner, by dividing the focal length of the zenith telescope by that of the telescope of the collimator, and multiplying the result by the power of the eye-glass, or by the quotient of ten inches divided by its focal length. Thus the focal length of the zenith telescope being forty inches, and the magnifying power ninety-nine times, the focal length of the eye-glass will be about four tenths of an inch. Then dividing forty inches by eight inches, the focal length of the telescope of the collimator, and this again by the power of the eye-glass, we have $\frac{40}{5} \div \frac{10}{0.4} = 125$ for the magnifying power exerted upon the diaphragm of the collimator.

The collimator I have described has a float 10.6 inches diameter; but I have had one constructed on a much smaller scale for Captain FOSTER, R.N. the float of which is only five inches in diameter, and its telescope about five inches long.

Some experiments have been made with this little instrument, the results of which have so far exceeded my expectations, that I think they may not prove uninteresting*.

* On taking down my collimator after it had been exposed for six months, in order to replace it by that made for Captain FOSTER, I found it very rusty, and the mercury very dirty. On taking out the float, it appeared that the mercury had adhered to it, so as to form a kind of coating like amalgam. These circumstances may have slightly affected the accuracy of the instrument. I have since discovered that by rubbing the float with chalk, and afterwards wiping it, the adhesion of the mercury is prevented.

Experiments with Captain FOSTER's Collimator.

Date. 1828.	Reading of the Microm. Divisions.	Inclination of the Collimator. Divisions.	Inclination in Seconds.	Reading at the Zenith. Divisions.	Zenith Point in Seconds.	Possible Error in Seconds.
Feb. 22.	90.7	3.35	"	87.35	43.67	-0.12
	84.0		1.67			
	89.0	1.15	0.57	87.85	43.92	+0.23
	86.7					
	87.0	0.50	0.25	86.50	43.25	-0.45
	86.0					
	87.3	1.45	0.72	88.75	44.37	+0.27
	90.2					
	86.0	2.80	1.40	88.80	44.40	+0.17
	91.6					
	87.0	0.50	0.25	87.50	43.75	-0.14
	88.0					
	85.0	2.35	1.17	87.35	43.67	-0.04
	89.7					
	84.0	3.50	1.75	87.50	43.75	+0.21
	91.0					
	81.0	5.00	2.50	86.00	43.00	-0.71
	91.0					
	85.4	4.80	2.40	90.20	45.10	+0.53
	95.0					
	84.3	5.85	2.92	90.15	45.07	+0.15
	96.0					
	83.0	5.50	2.75	88.85	44.42	-0.33
	94.0					
					Mean..	-0.02

Experiments by Captain FOSTER.

Feb. 23.	63.2	27.40	"	90.60	45.30	+0.09
	118.0		13.70			
	65.0	25.25	12.62	90.25	45.12	-0.01
	115.5					
	63.5	26.50	13.25	90.00	45.00	+0.14
	116.5					
	64.0	24.65	12.32	88.65	44.32	+0.02
	113.3					
	66.2	25.90	12.95	87.10	43.55	-0.28
	108.0					
	67.8	20.00	10.00	87.80	43.90	+0.08
	107.8					
					Mean..	+0.01

Experiments by Mr. HARVEY.

Date. 1898.	Reading of the Microm. Divisions.	Inclination of the Collimator. Divisions.	Inclination in Seconds.	Reading at the Zenith. Divisions.	Zenith Point in Seconds.	Possible Error in Seconds.
Feb. 26.	69.4 57.5 69.7 60.0 69.5 60.2 68.3 61.0 69.6 57.2 67.7 62.7 69.5 59.8 68.8 59.4 65.0 57.5 69.5 58.0	5.95 4.85 4.65 3.65 6.20 2.50 4.85 4.70 3.75 5.75	" 2.97 2.42 2.32 1.82 3.10 1.25 2.42 2.35 1.87 2.87	63.45 64.85 64.85 64.65 63.40 65.20 64.65 64.10 61.25 63.75	31.72 32.42 32.42 32.32 31.70 32.60 32.32 32.05 30.62 31.87	+ 0.35 - 0.17 + 0.02 + 0.13 - 0.43 + 0.34 + 0.14 + 0.29 - 0.67 + 0.62
					Mean..	+ 0.06

The following TABLE contains the Error of each of the preceding determinations of the Zenith Point, by Captain FOSTER's Collimator.

Not exceed- ing One Tenth of a Second.	Between One and Two Tenths.	Between Two and Three Tenths.	Between Three and Four Tenths.	Between Four and Five Tenths.	Above Five Tenths.
-.04	-.12	+.23	-.33	-.45	-.71
+.09	+.17	+.27	+.35	-.43	+.53
-.01	-.14	+.21	+.34		-.67
+.02	+.15	-.28			+.62
+.08	+.14	+.29			
+.02	-.17				
	+.13				
	+.14				

I leave the results of the little floating collimator to speak for themselves, and shall now return to my larger collimator before described.

On the manner of using the Vertical Floating Collimator in Astronomical Observations.

The instrument employed in the observations which I shall first detail, was the portable azimuth and altitude circle described by the Rev. F. WOLLASTON in his "*Fasciculus Astronomicus*," and was the property of the late D. MOORE, Esq. F.R.S. This circle is only one foot in diameter; the divisions are dots upon brass, and it is also divided by lines. Of the dots, though they are the divisions used, I cannot learn the history. Many of them are much injured, and some nearly obliterated.

The instrument is furnished with two microscopes, the micrometer heads of which are divided to two seconds; but I have attempted by the eye to estimate the readings to tenths of a second. The focal length of the telescope is twenty inches, and it magnifies about thirty times.

I shall now describe the manner in which the different adjustments of an altitude and azimuth circle may be readily effected by means of the vertical floating collimator.

To adjust the Line of Collimation.

The circle being placed in the meridian, and the vertical floating collimator over it, the meridian wire is to be brought to the angular point of the diaphragm of the collimator, by means of one of the foot-screws of the circle. The axis is now to be taken out of the Y's and reversed; when if the meridian wire is no longer on the angular point of the diaphragm, the space over which it has moved is equal to double the error of the line of collimation. Bisect this space by moving the meridian wire by means of the screws in its diaphragm, and the line of collimation will then be at right angles to the axis. This is proved by the meridian wire suffering no change of place when the axis is reversed*.

To place the Horizontal axis of the Circle at right angles to the Vertical axis.

Bring the meridian wire to the angular point of the diaphragm of the collimator by means of the foot-screw. Turn the circle half round in azimuth, and then note the distance of the meridian wire from the angular point. Half this distance is the error of the horizontal axis, or its deviation from perpendicularity to the vertical axis. Cause the meridian wire to bisect this distance by

* The operation just described, is nothing more than the process of adjusting the line of collimation of a transit instrument, the angular point of the diaphragm of the collimator serving instead of a distant terrestrial object. It may be done with great convenience by means of the horizontal floating collimator.

means of the screw which acts upon that Y which is opposite to the microscopes, and the horizontal axis will be perpendicular to the vertical axis. This adjustment is known to be perfect when the meridian wire remains upon the angular point after the circle has been turned half round in azimuth.

To place the axis of the circle parallel to the Horizon.

Lodge the iron pin of the basin of the collimator in one of the Y's, and by means of one of the foot-screws bring the meridian wire to the angular point of the diaphragm. Turn the collimator half round, and note the distance (if any) of the meridian wire from the angular point. Half this distance is the error of the axis or its deviation from horizontality. Cause the meridian wire to bisect this distance by means of the foot-screw, and the axis of the circle will then be parallel to the horizon. Examine this adjustment by turning the collimator half round; when if it is correct, the angular point will be equally distant on either side from the meridian wire, of which the eye will judge with considerable accuracy*.

These are all the adjustments that are necessary; and whoever has enjoyed the superior convenience and facility of effecting them by means of the floating collimator, will scarcely prevail on himself to return to the instability and uncertainty of a level.

Should it be necessary, the telescope of the collimator must be turned in its tube (the iron pin being lodged in one of the Y's) until the angles of the diaphragm can be bisected by the horizontal wire of the circle, when the instrument is ready to be employed in celestial observations. I have of course taken it for granted that the float has been adjusted by the application of weights to it, so that the angular point of the diaphragm remains either upon or very near the meridian wire, and does not depart far from the horizontal wire when the collimator is turned half round.

Before quitting the subject of adjustments, I may remark, that the vertical floating collimator affords the most perfect method of adjusting the line of collimation of a mural circle, or of placing it at right angles to the axis. But for this purpose it will be necessary to employ two of these instruments, one placed above, and the other below the circle. Bring then the meridian wire

* I scarcely need point out the value of this mode of adjustment as applied to a transit instrument. It insures the line of collimation describing a vertical circle, and is independent of any inequality in the size of the pivots.

of the mural circle to the angular point of the diaphragm of the upper floating collimator. Turn the collimator half round and bring back the meridian wire by adjusting the axis of the circle through half the distance it has appeared to move from the angular point. Look now into the lower collimator, and note the distance at which the angular point appears to be from the meridian wire. Turn this collimator half round, and remark whether the angular point is at the same distance as before on the other side of the meridian wire; and if the distance is not the same, halve the estimated difference by moving the meridian wire by means of the adjusting screws of its diaphragm. Repeat these operations until in both collimators the angular point appears to move to an equal distance on each side of the meridian wire on the collimator being turned half round, when the line of collimation will be at right angles to the axis, and the axis will by the same process have been placed parallel to the horizon.

It will be found convenient before the observation of the star is made, to ascertain that the illumination of the collimator is perfect, as it must be evident that it is important to determine the zenith point of the instrument as soon after the observation of the star as possible.

Should the star be at such an altitude as for the view of it to be intercepted by the collimator, the collimator must be moved as far as may be requisite along the beams. After the star has been carefully bisected, and before the microscopes are read off, the shutter of the observatory is to be closed, and the collimator is to be brought back to its place and turned half round. The microscopes are then to be read off and registered, and by the time this is completed, the collimator will be steady. The angles of the diaphragm are now to be bisected by the horizontal wire, and the collimator immediately turned half round. The divisions of the circle are then to be examined, and the microscopes read off and registered. Long before this, the collimator will again be steady, when the angles are again to be bisected, and the readings of the microscopes registered. This forms a complete observation; but it is desirable to repeat the operation for the determination of the zenith point, in order to preclude or detect error. The mean of the readings at the collimator will be the place of the zenith point upon the circle, and the difference between this and 90 degrees or zero, will furnish a correction to be applied with its proper sign to the reading at the star, for the purpose of obtaining its apparent altitude or its zenith distance.

The refractions used in the following Table, are those given by Dr. Young in the Nautical Almanac.

The resulting latitude of York Gate is $51^{\circ} 31' 20''.76$.

The mean of 40 observations of different stars in 1825, using the horizontal floating collimator and the same circle, gave $51^{\circ} 31' 20''.94$.

If we analyse the observations detailed in the preceding table, we may form an estimate: 1st, of the stability of the instrument, and the efficiency of its telescope and microscopes: 2dly, of the degree of accuracy with which a star has been bisected; and 3dly, of the equality or otherwise of the divisions of the circle.

On examining the column of the zenith point, we perceive that on the same evening the differences are but small: for example, on the 23rd of June the greatest difference from the mean was only $2''.25$. From which we may infer, that the support of the instrument suffered little change of position during that evening, and that the telescope and microscopes were of a power sufficient to determine this quantity.

On referring to the corresponding inclination of the collimator, we find it in the course of the evening to have varied considerably, without affecting the accurate determination of the zenith point. The cause of this variation I conceive to be the bridge having in the first instance been made very slight, and being consequently readily affected by change of temperature when the shutter of the observatory was opened.

If the bisection of a star could be accurately made, and there existed no uncertainty with respect to refraction, the same latitude would be given on different evenings by the same star; as its altitude is determined by reference to the same division of the instrument. But, we may see that in the preceding table these results differ. With α Herculis the greatest difference is $4''.34$, with α Ophiuchi $5''.34$, and with Antares the difference amounts to $6''.6$. From this we may conclude that, taking an unfavourable state of the atmosphere into consideration, the uncertainty in bisecting a star with this telescope may probably be about three seconds.

As the altitude of each star is referred to a different division of the circle, if the mean of the latitudes given by each star be taken, and these means be compared together, they ought, if the circle is well divided, to agree, and their difference will give some idea of the error in this respect. The observations of each star have not been sufficiently numerous to determine this with accuracy; but I am inclined to think it probable that the error of such points of the circle as have been used does not exceed three seconds.

It is possible that the dots to which the zenith point is referred may be in error; in which case, all the altitudes will be affected to an amount equal to that error. This however is destroyed by reversing the circle, observing with the face the contrary way, and taking the mean of the results in both positions.

If the circle should be upon stone pillars, or so circumstanced that there should be room for the floating collimator below it, it may be employed in that position, its legs serving as a support. The only alteration then necessary will be to invert its telescope, placing the object-glass uppermost, and illuminating the diaphragm from below by means of a mirror attached to a small mass of lead, which may be placed on the ground. The telescope of the circle will then look into that of the collimator downwards; and this, if the circle should not be too high, may sometimes be the more convenient method.

Of the Application of the Vertical Floating Collimator to a Zenith Telescope.

So much has already been said which is appropriate to this subject in describing the adjustment of the collimator, and the determination of the zenith point, that little remains to be added. The Newtonian telescope was the instrument employed; and in the two first observations of γ Draconis, its wooden frame not being finished, the telescope was brought nearly in the direction of the zenith by three small wedges of wood placed under the mirror end, and forming, it must be confessed, a very frail support. The focus of the telescope, too, had not been accurately adjusted, and the moveable wire was placed parallel to the equator merely by estimation. Between the two observations, the telescope was removed; they are recorded, however, as matter of curiosity, but I have not included them in the mean of the observations detailed, though they would not have vitiated the result.

Should the star pass the meridian at night, it will of course be requisite to illuminate the wires of the micrometer in the usual manner. The collimator having been moved along the beams which support it, out of the way of the telescope, the star is to be carefully bisected by the moveable wire, the collimator to be brought back and turned half round, and then the reading of the micrometer at the star to be registered. The angles of the diaphragm are now to be bisected, and the collimator having been turned half round, the divisions of the micrometer are to be recorded. Lastly, the angles of the diaphragm are again to be bisected, and the reading of the micrometer registered. This com-

pletes the observation, which altogether requires about five minutes. The mean of the readings at the collimator will give the zenith point; and the difference between the zenith point and the reading at the star, will be the star's zenith distance in divisions of the micrometer to be converted into seconds.

It will be advisable to repeat the determination of the zenith point as directed in observations with the circle, to guard against error.

If the aperture of the zenith telescope should be sufficiently large to render the loss of light from the interposition of the telescope of the collimator of no consequence, it may not be necessary to remove the collimator; but the iron cover B, (see Plate XIII.) may be raised, and the star observed through the opening in the support of the collimator. The cover is then to be replaced, and the zenith point determined in the usual manner.

The diameter of the opening of my collimator being four inches, and the extreme diameter of the telescope an inch and a half, the loss of light would be about one seventh part of the whole; I have not yet tried this method of observing, and perhaps it may be found that the bridge of the collimator (which for this purpose should be thin and deep) may occasion some distortion in the image of the star.

TABLE of Observations with the Zenith Telescope.

Date. 1827.	Reading at the Star. Divisions.	Reading at the Collimator. Divisions.	Zenith Point. Divisions.	Star's Zenith Distance. Divisions.	Star's Zenith Distance. Seconds.	Sum of Corrections.	Zenith Distance reduced to Jan ^y 1827.	Remarks.
July 9.	367.5	278.0 } 385.5 } 294.2 }	331.75	35.75	17.87	+13.17	31.04	Bad image, and no- thing well adjusted.
Aug. 31.	247	141.0 }	217.6	29.4	14.7	+25.51	40.21	Bad image, and no- thing well adjusted.
Sept. 1.	237	237.5 } 235.2 } 196.5 } 202.0 } 158.0 } 162.0 } 142.7 } 137.5 }	217.8	19.2	9.6	+25.61	35.21	Excellent.
5	169	158.0 } 162.0 } 142.7 } 137.5 }	150.05	18.95	9.47	+25.99	35.46	Star past the meri- dian.
6	89	74.0 } 77.0 } 64.5 } 66.6 }	70.52	18.48	9.24	+26.07	35.31	Tremor.
Oct. 24.	63.7	21.2 } 31.8 } 42.0 } 48.4 }	35.85	27.85	13.92	+24.14	38.06	Focus bad, and ad- justed a moment be- fore the observation.

CAPTAIN KATER'S DESCRIPTION OF

Observations with the Zenith Telescope. (Continued.)

Date. 1827.	Reading at the Star. Divisions.	Reading at the Collimator. Divisions.	Zenith Point. Divisions.	Star's Zenith Distance. Divisions.	Star's Zenith Distance. Seconds.	Sum of Corrections.	Zenith Distance reduced to Jan' 1827.	Remarks.
Oct. 24. Continued.		21.0 } 31.7 } 44.8 } 49.0 }	36.64					
		21.9 } 32.6 } 45.0 } 49.5 }	37.25					
31	81.5 } 79.0 }	18.7 } 10.7 } 91.8 } 88.8 } 18.8 } 11.8 } 86.7 } 85.5 }	52.5	27.75	13.87	+22.83	36.70	
		21.0 } 12.3 } 86.5 } 88.0 }	51.95					
Nov. 2.	145.6	143.0 } 160.7 } 82.5 }	120.87	24.73	12.36	+22.42	34.78	Doubtful.
5	62	97.3 } 47.5 } 0.0 }	35.87	26.13	13.06	+21.80	34.86	Line not well ad- justed.
12	42.7	72.0 } 24.0 } 26.5 }	15.05	27.65	13.82	+20.17	33.99	Cloudy. Stars scarce- ly visible.
22	137	19.7 } 7.0 } 7.0 }	97.92	39.08	19.54	+17.45	36.99	Flying clouds.
		118.0 } 112.0 } 83.0 }	97.55					
24	135.8	78.7 } 119.8 } 110.7 } 81.0 }	97.27	38.53	19.26	+16.86	36.12	
		78.7 } 78.0 } 119.8 } 114.0 }	97.95					
29	110.5	80.0 } 78.0 } 60.0 }	68.5	42.0	21.0	+15.40	36.40	
		93.0 } 75.0 }						

Observations with the Zenith Telescope. (Continued.)

[illegible]

In the preceding Table I have given, as is my practice, every observation which has been made:—the first two, as I before said, are inserted merely as matter of curiosity, though their mean happens to be the same as that ultimately adopted. The last observation I consider inadmissible, as the weather was so hazy that the star was scarcely visible, and that only at intervals. The observation of the 24th of October I feel no hesitation in rejecting, from the disturbance the instrument might have suffered from adjusting the focus of the telescope to the star the moment before the star was bisected, and the consequent hurry in which the observation was made. I therefore consider $35''.67$ as the mean, which is nearest the truth. The corrections for aberration, &c. &c. have been taken from the Tables just published by the Astronomer Royal.

It is far from my wish that the astronomical part of the observations here given should be considered as proofs of the utmost accuracy which a telescope so employed is capable of attaining; for it may readily be conceived that had the telescope been firmly fixed by stone- or brick-work, and time taken to place the moveable line of the micrometer accurately parallel to the equator, the bisection of the star might probably have been effected with a much greater degree of precision.

The focal length of the telescope employed was only forty inches, and the scale of the micrometer, or the number of divisions which are equal to a second, it has been remarked, is in proportion to the focal length of the telescope. The shortness of my telescope therefore, may be justly conceived to have taken somewhat from the accuracy of the results; and I ought also to mention, that by far the greater number of these observations, namely, those from the 24th of October, were made in the day time.

Notwithstanding these considerations, the power of the floating collimator is such, that I shall venture to compare the preceding observations with those made under the most favourable circumstances, and with an instrument which is justly esteemed the most perfect of its kind ever constructed, the Zenith Sector belonging to the Board of Ordnance*.

In a series of observations, one or two may perhaps be found which may accidentally differ considerably from the mean of the whole. But the difference

* This instrument is furnished with an achromatic telescope of eight feet focal length.

between such insulated observations and the mean, cannot be considered as a measure either of the power of the instrument, or of the skill of the observer. These will be more justly estimated by taking the difference between the general mean, and the mean of such observations as exceed and fall short of it*.

In this manner I have examined the observations made with the zenith sector at the Stations of the Trigonometrical Survey, and the following are the results :

Number of Observations.	Difference from the Mean.		
7	+0.49	-0.74	β Draconis.
7	+0.88	-0.65	
9	+0.81	-1.02	γ Ursæ.
8	+0.39	-0.71	β Draconis.
8	+0.52	-0.30	γ Draconis.
7	+0.42	-0.44	
7	+0.28	-1.70	β Draconis.
7	+0.67	-0.89	
8	+0.85	-0.84	γ Draconis.
9	+0.17	-0.57	51 Draconis.
9	+0.68	-0.55	α Cygni.
8	+0.33	-0.63	ϵ Cygni.
Mean.. 8	+0.54	-0.75	

The above may be considered as a fair representation of the power of the zenith sector; and I may add, that I have selected those sets which consist of the greater number of observations, and have confined myself to such stars as passed within two degrees of the zenith, to avoid any possible error which might have arisen from uncertain refraction.

I shall now divide the fifteen zenith distances obtained by means of the floating collimator into two sets, consisting of seven and of eight observations each, in order that they may be similarly circumstanced with the observations made with the zenith sector. Proceeding in the same manner as before, we obtain the following results :

Number of Observations.	Difference from the Mean.	
7	+0.48	-0.65
8	+0.40	-0.66
Mean.. 8	+0.44	-0.66

* I am indebted for this suggestion to Dr. WOLLASTON.

The comparison then between the zenith sector and the zenith telescope used with the vertical floating collimator, will stand thus :

Mean of errors by the zenith sector $+0''.54$ and $-0''.75$
 Mean of errors by the zenith telescope used
 with the floating collimator* $+0''.44$ and $-0''.66$

I shall now proceed to deduce the latitude of York Gate from the observations made with the zenith telescope.

The polar distance of γ Draconis for January 1827,
 with which I have been favoured by the Astro-
 nomer Royal from the mean of 296 observations,
 was $38^\circ 29' 14''.64$; which gives for the zenith di-

distance of γ Draconis at Greenwich	$0^\circ 2' 6''.36$
Add zenith distance at York Gate	$0 \ 0 \ 35 \ .67$
Difference of latitude between Greenwich and York Gate	$0 \ 2 \ 42 \ .03$
Latitude of Greenwich	$51 \ 28 \ 38 \ .96$
Latitude of York Gate	$51 \ 31 \ 20 \ .99$

* It may be remarked, that if the first eight observations and the last seven had been taken to form the two sets, the result would have been less favourable to the zenith telescope. But in the last seven observations there would then have been only a single observation less than the mean ; and this, as I have before said, is an inadmissible case for comparison. Were this observation excluded, and the two sets made to consist of eight and of six observations, the resulting differences from the mean would have been far more favourable. The following is a view of the results.

Number of Observations.	Difference from the Mean.	
8	$+0.97$	-0.58
7	$+0.29$	-1.77^*
Mean . 8	$+0.63$	-1.17
8	$+0.97$	-0.58
6	$+0.19$	-0.19
Mean . 7	$+0.58$	-0.38

* One observation only, less than the mean, and result therefore inadmissible.

As any difference in the tables of refraction employed will equally affect the latitude and the zenith distance deduced from it, no correction is necessary on this account.

We have then for the latitude of York Gate,

By the azimuth and altitude circle and the horizontal floating collimator	} 51° 31' 20".94
By the same instrument and the vertical floating collimator	} 51 31 20 .76
By the zenith telescope and the vertical floating collimator	} 51 31 20 .99
Mean	<u>51 31 20 .90</u>

In my description of the horizontal floating collimator, I have recommended it to be employed in an observatory as a *fixed point*; its zenith distance being determined by means of the vertical floating collimator. For this purpose the box should be of cast iron, the openings in the ends of the box closed by pieces of plane glass, and the cover rendered air-tight. We have seen that the error in the vertical floating collimator is scarcely appreciable, though the mercury and float are agitated by turning the instrument half round; and it is not too much to anticipate, that where there is no such cause of disturbance, the horizontal floating collimator will suffer no change of inclination. This, however, may readily be ascertained by experiment.

If I have succeeded in the object of this paper, I shall have demonstrated that the vertical floating collimator is an instrument capable of determining the zenith point with a precision hitherto unknown; that by its aid a meridional observation of an altitude or of a zenith distance may be completed, not only the same evening, but within the space of a very few minutes, and that too, without the necessity of turning the circle in azimuth. These are advantages which no other method of observing affords, and which astronomers well know how to appreciate. If to these be added the facility with which the floating collimator may be constructed, the ease with which it is used, and its general applicability to all astronomical circles, whether small or of large dimensions, it may not perhaps be too much to infer that ere long the use of the level and of the plumb-line in celestial observations will be wholly abandoned.

XIII. *On the height of the Aurora borealis above the surface of the earth ; particularly one seen on the 29th of March, 1826.* By JOHN DALTON, F.R.S.

Read April 17th, 1828.

APPREHENDING that the Royal Society will favourably receive accounts that have a direct tendency to determine the height of that interesting phenomenon, the Aurora borealis, I have been induced to transmit some observations that were made upon a very remarkable one, which appeared in the evening of the 29th of March, 1826. From some recent observations, an opinion seems to be entertained by some writers, that the aurora is not so high as has generally been estimated ; but it is only from facts and observations such as the following, I conceive, that any near approximation to the true height can be obtained.

The aurora borealis above mentioned, was of a kind very rarely occurring. It assumed the appearance of a rainbow-like arch, stretching across the mid-heaven, at right angles to the magnetic meridian. It was subject to very little change of position for an hour or more, and therefore afforded time to observe the angle of its elevation above the horizon. In the period of five years' observations at Kendal formerly, above one hundred appearances of the aurora occurred to me, and only one of the kind just described. I had not an opportunity of seeing the one which is the subject of this paper, but it was seen here (at Manchester) by a friend of mine about 9 o'clock on his returning home from a visit to me. He did not indeed observe the luminous arch, either from its having vanished, or from the obscurity of our atmosphere ; but he remarked some beams or corruscations in the north-western hemisphere, of a low altitude ; and not having seen an aurora for a long time, he induced the family at home to go out and catch a glimpse of the phenomenon, now much more rarely seen than formerly.

A few days afterwards I accidentally noticed a paragraph in the Lancaster Gazette describing the luminous arch of the aurora, as well as the accompanying appearances; and as such a striking and unusual phenomenon could not fail to attract general attention, I examined the provincial newspapers and other periodicals of the time, and took occasion soon after to make inquiries personally, or by writing, of such individuals of judgement as had seen the phenomenon in various places near the line of the magnetic meridian. The result was, a collection of a more complete and extensive series of observations than was ever before made, in all probability, towards determining the height of the luminous arch of the aurora.—I shall now proceed to detail some of the particular observations.

The accounts represent the arch to have been seen in places 170 miles distant in a north and south direction, and forty-five miles distant in an east and west direction, comprising an area of seven or eight thousand square miles; but it must have been much more extensively visible, as in most cases the writers of the different accounts describe their situation as central with regard to the phenomenon. It was seen at Edinburgh and Leith, Kelso, Jedburgh, and Hawick in Scotland; at Carlisle, Penrith, Keswick, Cockermouth, and Whitehaven in Cumberland; at Kendal and at Kirkby-Stephen in Westmorland; at Lancaster, Preston, Warrington, and Manchester in Lancashire; and at Doncaster in Yorkshire. Descriptions of the phenomena as seen at most of these places were immediately given in the newspapers of Lancaster, Kendal, Carlisle, Whitehaven, Kelso, &c., and some of these accounts were copied into the London papers soon after.

All the accounts that I have seen from places between Lancaster and Edinburgh, as well as at these two places, agree that a luminous arch was first seen about 8 o'clock in the evening; that it continued without much motion for an hour nearly, and then gradually vanished, leaving the northern sky illuminated as usual after an aurora borealis of the common kind: so that it seems impossible to doubt that the same arch was seen at all the places of observation, and at the same time.

A good description of the phenomenon was published by Messrs. Coldstream and Fogg in the Edinburgh Journal of Science for June 1826: it is as follows:—

“ March 29th. Immediately after the fading of the evening twilight, at 8^h 15^m P.M., a bright luminous ray was seen to rise from the eastern horizon, gradually to extend itself towards the zenith, and thence towards the western horizon, presenting, when completed, the appearance of an arch of silvery light, similar to that seen here on the 19th March, 1825.

“ When first formed it was a few degrees to the north of the zenith of this place; the light in the centre was rather diffuse; its edges were irregular; and the western limb had, as it were, a plumose appearance. It soon evinced a decided motion towards the south, and in a few minutes reached our zenith. Its edges were now sharply defined, and throughout its whole course it was nearly uniform in appearance and breadth; the intensity of its light in the zenith had increased, while in the same quarter the breadth had considerably diminished.

“ The direction it now had was very nearly at right angles with the magnetic meridian.

“ At $\frac{1}{2}$ past 8, faint beams of the aurora began to rise from the northern horizon, and at one time promised to form a splendid display; but the corruscations never became very vivid; they were not rapid in their motions, and did not flit along the horizon.

“ The arch still continued its motion towards the south, and in 15 minutes passed through a space of about 20°. Its southern edge reached a point about 24° or 25° south of the zenith, beyond which it did not go. The light now became gradually fainter, and at length disappeared.

“ Meanwhile the aurora in the north continued to play, but with no increase of vividness. For some minutes, soon after 9 o'clock, we observed broad bands of light, having their longer axes (which generally subtend angles of about 18° or 20°) parallel with the horizon, darting with great velocity across the illuminated space from east to west and from west to east. These formed, ran their course, and vanished in a moment; they had no vertical motion, but they appeared at various degrees of elevation, never higher however than 30°. Soon after this interesting (and perhaps unusual) display, the beams disappeared, and nothing was left but a diffuse luminousness along the horizon.”

At Jedburgh, Hawick and Kelso, places about forty miles south of Edinburgh, the phenomena were much the same as above, as appears from the

Kelso Chronicle. (See also the London Courier, April 7th, and other of the daily papers.) At Jedburgh the arch is said to have commenced at 8^h 15^m on the W. by S. point of the horizon, to have passed south of the star Aldebaran, between Castor and Pollux, and over Arcturus; its altitude 60° from the S.; waves of light seemed to run along the arch. At 8^h 30^m the whole advanced 20° to the S. At Hawick it was at first 20° S. of the zenith, and at 8^h 40^m it was stationary at 37° S. of the zenith; the arch passed 6° N. of Arcturus, 7° S. of Cor. Caroli, 6° N. of Coma Berenices, through the hind foot of Ursa Major, 4° N. of Asellus Borealis, 6° S. of Pollux, through the head of Monoceros, through the three stars in Orion's girdle, and 1° S. of Rigel. From this it would seem that the arch, instead of appearing low in the north from the last-mentioned places, as it must have done if situated only five or even ten miles above the earth's surface, appeared as far to the south of the zenith as at Edinburgh, or rather further. This latter it could not do; and in such circumstances it is reasonable to allow a difference of a few degrees in the estimates of altitudes of arches neither well defined nor absolutely fixed, and possessing several degrees of breadth; but it clearly shows the arch was not low. The author of the Hawick account signs, GIDEON SCOTT.

At Carlisle, seventy-five miles S. of Edinburgh, the phenomena were much the same as in the preceding accounts. See the two weekly newspapers of that city.

About Cockermouth, twenty-five miles S. of Carlisle, I conversed with many persons who had seen the phenomena. One young gentleman, Mr. HARRIS, had committed to paper at the time some notes upon it, with which he favoured me. According to these, he first saw the arch at 7^h 45^m P.M., it extended nearly from the western to the eastern horizon, through the W. part of the head of Orion, over Castor and Pollux, S. of Ursa Major, and ended in Corona Borealis; it continued with little variation in its situation till near 10 o'clock. At first the west end of the arch was most luminous, and finally before it vanished the east end was the most brilliant. The eastern end waxed and waned frequently. The sky was very clear, a few streamers appeared low in the horizon.

At Keswick, about twelve miles east of Cockermouth, the appearance was described as follows, in a letter to me from Mr. OTLEY. This gentleman is

known to the public by an elegant little description of the Lakes and Mountains of the North of England, and is familiar with observations relating to meteorology, and to the angles of elevation of objects. "About 8 P.M., a luminous arch appeared very brilliant; the outside of the curve seemed a little south of the zenith. The eastern end tapered to a point above the horizon; the western end was broader, and lost in a cloud which rested on the mountain. It disappeared about 10 o'clock."

At Whitehaven, one hundred miles from Edinburgh, and a few miles more to the westward, a minute description of the phenomenon was given in one of the newspapers of that place, by Mr. HOLDEN, lecturer on astronomy, who happened to be there at that time. At 8^h 45^m the east leg of the arch covered α Coronæ Borealis, the northern edge of the bow touched Castor near its greatest altitude, and the west leg went over the three small stars marked λ in the head of Orion. The breadth at greatest altitude was 4° 40', but tapered down to the horizon, where it was not more than one-fourth of that breadth. The east leg was 15° north of the east point, and about the same number of degrees south of the zenith; and the west leg was 15° south of the west point. At 9^h 8^m the arch had moved southward, Pollux touched the north of the bow, the west leg extended over α Orionis, and the east leg was still upon α Coronæ Borealis, but this star had been moving in its apparent track by the earth's motion for the space of twenty-three minutes. He saw several small clouds move before and cover portions of it for a few seconds of time.

From Kirkby-Stephen, about forty-five miles east of Whitehaven, a good description of the phenomenon is given in the Westmorland Gazette. The mean breadth of the luminous arch exceeded that of the rainbow, the vertex broader, the extremities narrower, and the light more dense. The arch gradually faded about 10 P.M., having existed nearly two hours. The light was white and transparent. Position at 9 P.M., the arch of a great circle from E. 25° N. through the zenith to W. 25° S. At first the eastern extremity of the arch was near β Herculis, thence it passed the north side of Corona Borealis, through the midst of the seven stars in the Great Bear, over the zenith to the north of Castor, exactly over Bellatrix, after which it contracted to a point in Eridanus just above west. This writer makes no mention of any appearance of the common aurora borealis at the same time.

Accounts from Penrith were much the same as the preceding ones ; but I had no opportunity of seeing any of them.

At Kendal, which is 110 or 115 miles S. of Edinburgh, and very nearly on the same magnetic meridian, (consequently the same part of the arch must have crossed the meridian at both places,) the following is a description of the phenomena as they appeared there, and might have been adopted with very little error, it should seem, for that at Edinburgh or any one of the intervening places, except as to the altitude of the summit of the arch. "A most magnificent meteor was observed here between 8 and 9 o'clock. The appearance was that of a luminous arch, stretching quite across the heavens. Its direction was that of the magnetic east and west, intersecting the magnetic meridian at right angles. At the same time a splendid light was observable in the northern horizon. This meteor was similar in some particulars to one which appeared a few years ago." [Query in 1819:] "The arch itself appeared like two frustums of cones, with the less extremity in the horizon, and their bases meeting in the zenith. The densest parts of the bow were those near the horizon, and the west end the denser of the two."

The phenomenon was seen at Lancaster, twenty miles S. of Kendal, and 130 miles S. of Edinburgh ; it was described in the next Lancaster Gazette, but without being specific as to the altitude of the centre of the arch. Inquiry having been made of an intelligent medical gentleman who had seen it, he described the luminous arch as extending from east to west across the zenith, the light increasing in intensity from the arch of the zenith to the line of the horizon ; there were those faint corruscations which usually attend an aurora borealis. This was about 8 o'clock ; at 10^h 30^m P.M. there was a luminous appearance along the northern horizon.

The aurora was seen at Preston, twenty miles S. of Lancaster ; but I have not been able to learn the particular appearances at that place. It was also seen at Doncaster in Yorkshire, but I have not noticed any description of its appearance at that place.

At Warrington the luminous arch was seen by a friend of mine, Mr. JOSEPH CROSSFIELD, who was so obliging as to give me interesting information on the subject, both verbally and by writing. He saw the arch about 9 o'clock, or between that and 10, in company with two other persons, to whom he pointed

it out at the time. At the first glance he took it for the milky way, but soon discovered his mistake. The direction of the arch was from W.S.W. to E.N.E., passing to the north of the zenith. The western branch was longer and more brilliant. He saw no northern lights at the time, neither did he apprehend the phenomenon was connected with them. On elevating the pole of a celestial globe till the axis passed through a series of angles with the horizon, I desired him to fix upon an elevation which he judged most nearly to coincide with the elevation of the centre of the luminous arch. On examination, the angle was found to be 61° . I then fixed the axis at 70° ; this he was almost certain was too high. When it was fixed at 50° , he was still more certain it was too low.

The aurora was seen at Manchester, as has been stated; but it does not appear to have attracted much attention at this place. I have not been able to trace any account of the phenomena having been seen further south.

These are all the material observations I have collected; from which it must appear that the descriptions every where given evidently apply to the same luminous arch. In proceeding from north to south we find the arch gradually advancing in altitude, always crossing the meridian to the *south* of the zenith, till we arrive about Kendal, at which place it crossed nearly in the zenith, and when at Warrington its culminating was to the *north* of the zenith. It is further remarkable, that in all the places the arch seemed to terminate nearly in the magnetic east and west, or at two opposite points of the horizon; these facts indicated the great height and extension of the arch.

In order to apply the data to calculate the height of the arch, it is evident that observations at the extremities of the magnetic meridians are to be preferred, and those on or near the same meridian, all other circumstances being the same. Unfortunately, the Edinburgh and Hawick observations do not harmonize together: however, those at Jedburgh, a place nearly of the same latitude as Hawick, seem to show that both the others are wrong, or rather perhaps, that they had not been cotemporary with each other and the rest of the observations. The Hawick altitude is probably too low, and that at Edinburgh considerably too high.

In this uncertainty we may be allowed to take the observations at Whitehaven and Warrington as guides. Those places are very nearly on the same magnetic meridian; they are distant eighty-three miles, giving an extensive

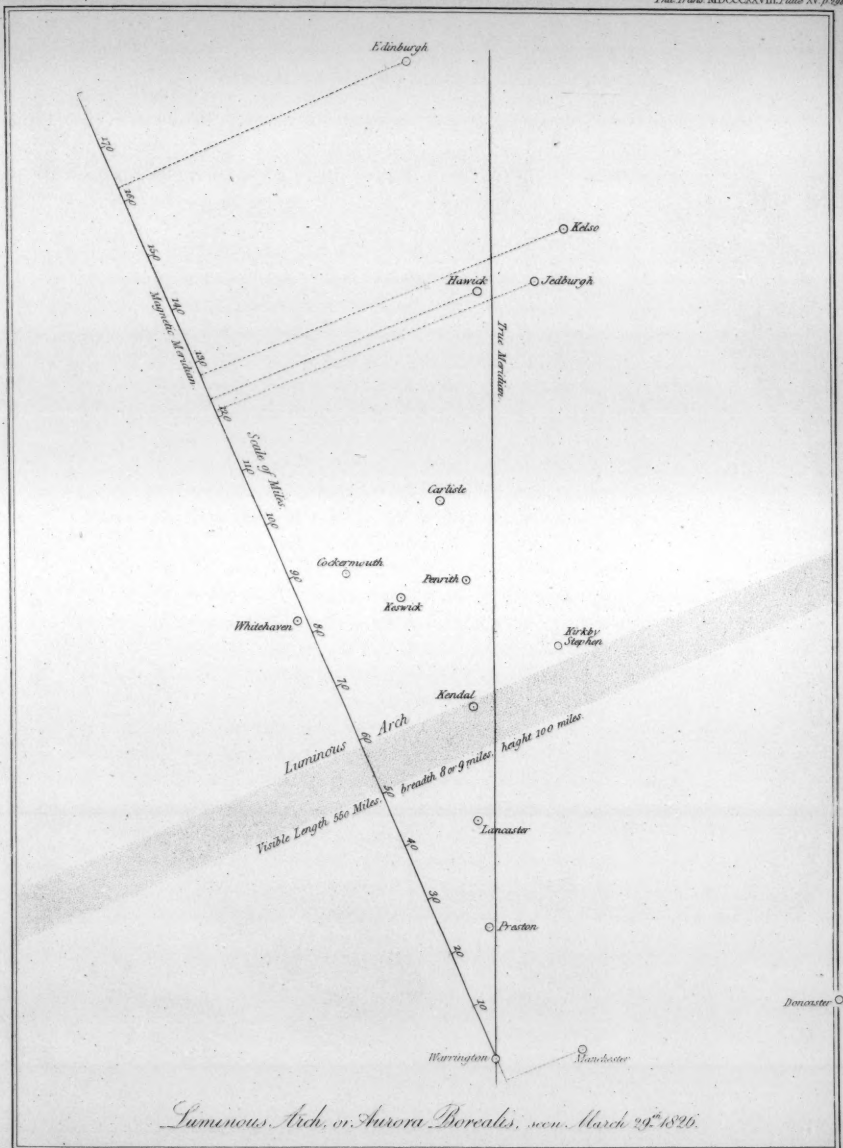
base : the observations were nearly cotemporary, and made on the same part of the arch, the altitude at Whitehaven being 75° from the south, and that at Warrington 61° from the north. From these data, I find the height of the arch very nearly one hundred miles above the earth's surface, and its position vertical about Kendal and Kirkby-Stephen, which accords well with the observations at those places. This conclusion is corroborated by the observations at Jedburgh and Warrington, where, if we take the angles of elevation at 60° and 61° respectively, and the distance on the magnetic meridian 120 miles, the height will be found between 100 and 110 miles. But, lastly, if we assume the angle at Edinburgh to be correct at 65° , and that at Warrington at 61° , the height comes out 150 or 160 miles, and its position vertical about Carlisle, which is in opposition to the general tenor of the rest of the observations.

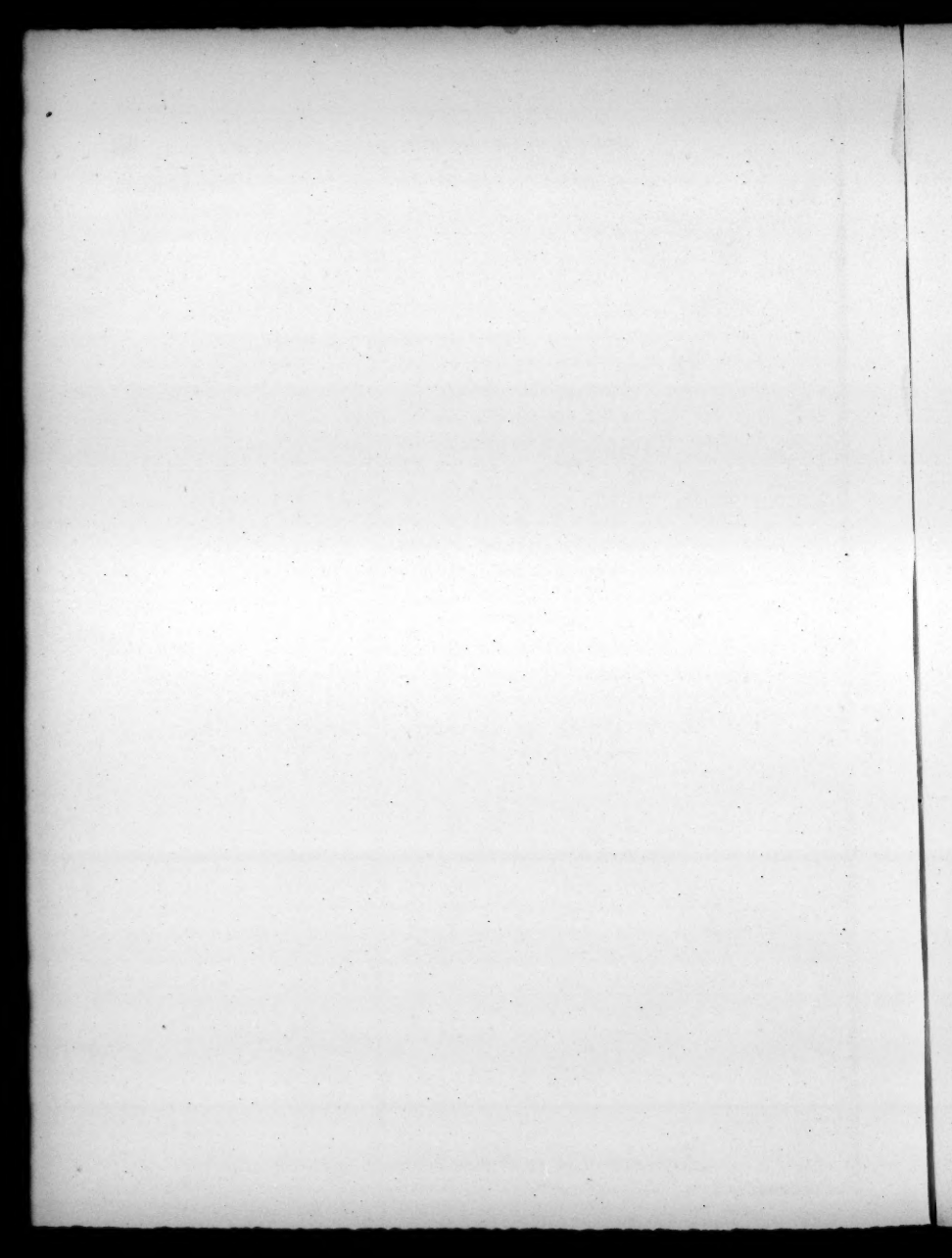
As for the heights of the streamers or vertical beams seen low in the north, we have no sufficient data for determining it. But it is evident that the beams which were seen low at Edinburgh were the same as those seen still lower at Cockermouth, Kendal, Lancaster, and Manchester, at which last place the angle was about 10° as my informant says. Now an object elevated about 25° from the north at Edinburgh would apparently be 10° or 12° at Manchester, if its real height were about one hundred miles above the earth's surface.

On the whole, I think it is fairly to be inferred that the height of the arch could not differ much from one hundred miles ; and that its breadth would be eight or nine miles, and its visible length in an east and west direction, from any one place, would be about 550 miles. (See the accompanying figure.)

Observations on other Auroræ.

The height of a luminous arch calculated by the late Mr. CAVENDISH, F.R.S. in the Phil. Trans. for 1790, is entitled to notice. It was found to be betwixt fifty-two and seventy-one miles. The observations, however, were made at too small a distance from each other to admit of precision. A base of at least forty or fifty miles seems necessary, where the object to be measured is generally neither steady nor well defined.





The luminous arch seen at Keswick and Kendal by Mr. CROSTHWAITE and myself, on February 15th, 1793, was calculated to be 150 miles high ; but this was from a base of only twenty-two miles. (See my Meteorological Observations and Essays, page 69.)

Dr. THOMSON has given a brief history of the Aurora borealis in the Annals of Philosophy for 1814, Vol. IV. He has copied a table from BERGMAN, being estimates of the heights of about thirty auroræ observed during the last century, calculated from observations made by different persons in various places. According to these results, the auroræ would seem to be of variable heights, from 130 to 1000 or more miles. The places of observation are often unsuitably situated ; and the data from which the calculations were made not being given, I apprehend the great differences in the heights arise more from defects in the observations than from real differences.

In the same volume Mr. LONGMIRE gives a description of a luminous arch seen at Troutbeck near Kendal, on the 11th of September, 1814. It was similar to that above described, and was most extensively seen : namely, at Glasgow, Dumfries, and Annan in Scotland ; at Dublin and Newry in Ireland ; and at Whitehaven, Carlisle, Kendal, Lancaster, Warrington, and Liverpool in England. It was accompanied with the usual appearances of the aurora borealis, or streamers distant in the north. The observations are insufficient for calculating the height. I find in my journal the aurora was noticed at Manchester that evening, but no particulars are given. Mr. LONGMIRE mentions a similar arch seen at Kendal and Dublin on the 17th of April the same year. An aurora was seen in London at the same time. (Annals of Philosophy, Vol. III. p. 400.)

1819, October 17th.—A remarkable aurora borealis was seen this evening in very distant parts of England and Scotland. Mr. ORLEY of Keswick first drew my attention to this, by communicating the notes he made at the time upon it, on the occasion when he favoured me with his remarks upon that of the 29th of March, 1826. After which I collected such other accounts as I could meet with from the journals of the time. The series of observations is as follows :—

Annals of Philosophy, Vol. XIV. p. 472. Account from Newton-Stewart, (Scotland,) October 18th.—“ A singular and beautiful phenomenon appeared

in our atmosphere here last night (17th), about 8 o'clock : it was a bow or arch of silvery light stretching from east to west, and intersecting the hemisphere [meridian] at a few degrees to the southward of the zenith. After it had remained very bright for twenty minutes or so, dark blanks were first observed to take place here and there, and then, after expanding a little in breadth and shifting for a short way further to the southward, it disappeared. Some time before its appearance the atmosphere had been very cloudy ; but when it was formed the sky was free from clouds, except towards the horizon to the westward and northward, where they hung very dark and heavy.—It was strikingly different from any of the usual forms of the boreal lights, which too were seen very vivid in the course of the evening."

Keswick. Mr. OTLEY's account :—"About 7 P.M. (the 17th), a dense cloud appeared in the horizon to the N.N.W. bounded by a bright line, the rest of the heavens being starry. Presently beams of an aurora began to shoot towards the Great Bear. About 8 o'clock a luminous arch extended from west to east ; the crown of the arch at first appeared to me a little to the north of the zenith, and after some time to the south of it, and again more northerly before it disappeared, which it did suddenly, a few minutes after 9 o'clock."

Manchester.—I have an account in my journal of an aurora seen here the same evening, but no particulars are given.

London.—The aurora was seen in and about London the same evening. (See pages 478 and 480, Vol. XIV. *Annals of Philosophy*.)

Gosport.—In the same volume of *Annals*, page 395, there is an account of the same aurora as seen at Gosport Observatory, Hampshire, on that evening by Dr. BURNEY. The following is an extract : "On the 17th instant, at 7 P.M. a light about 30° on either side of the magnetic north point appeared in the shape of a luminous arch whose apex was 18° above the horizon." He then describes several beams of the common aurora which successively appeared and traversed about for a time chiefly within the arch, and then vanished and were succeeded by others. After which, he adds : "Soon after this (9 o'clock) the luminous arch in the northern hemisphere entirely disappeared, and some haze collected near the horizon."

Gosport and Keswick are very nearly under the same magnetic meridian, and 265 miles distant. Newton-Stewart is N.W. by W. of Keswick, distant

about sixty-five miles, but only thirty-five miles in a meridional direction. Now I imagine it will be allowed that an extraordinary luminous arch seen at Newton-Stewart to cross the meridian a few degrees south of the zenith, and to continue from 8 to near 9 o'clock, nearly in that position, must have been the arch seen at Keswick at the same time to cross the meridian in like manner from east to west, and to pass nearly through the zenith. It may well be supposed, then, that this arch crossing through the zenith at Keswick would have a very diminished altitude if seen at Gosport, 265 miles south. From the account I have extracted, it appears that a luminous arch was seen there at the same time it was seen at the other places, and crossing the meridian at right angles, only its altitude 18° from the north, instead of being in the zenith, as at Keswick, or a few degrees south of it, as seen in Scotland. And further, the arch vanished at all the places at the same time. It scarcely admits of doubt, then, that these arches were all one and the same. By calculation from the data at Gosport and Keswick, I find the height of the arch above Keswick to be 100 or 102 miles; from which the angle of elevation from Newton-Stewart must have been 71° from the south, or the zenith distance of the arch 19° .

A luminous arch was seen at Kendal on the 27th of December, 1827, of which my friend Samuel MARSHALL was so good as to write me a circumstantial account. It was first seen at ten minutes past 6 in the evening, being an arch between the magnetic east and west, and passing through the zenith. It was broadest in the zenith, and it was more condensed in the eastern extremity than in the western. Another parallel arch appeared about 20° north of the former, of rather less intense light; and the northern horizon was luminous as usual on such occasions. After ten minutes or more, the arches advanced each of them to the south 20° with their centres. The appearance lasted about half an hour. A few streamers were seen in the east, which moved slowly northward. Mr. MARSHALL thinks the appearance would have been splendid if the moon had not shone at the time: a halo round the moon vanished when the bow approached it. I observed a halo round the moon at Manchester that evening.

Mr. BUCHAN, a gentleman accustomed to meteorological observations, had mentioned his having seen a similar arch at Manchester on that evening; but apprehending it might only have been a local phenomenon, I did not inquire

particulars till I received the above account from Kendal. Mr. BUCHAN informs me he saw a luminous arch that evening, about 9 o'clock; the arch was highest to the west of the meridian, and its altitude was very nearly the same as the north pole, just under which it passed; he estimates it at 53° , and thinks it could not be above 1° more or less. As this observation was not contemporary with that at Kendal, nothing certain can be deduced from them, but it may not be amiss to observe that an object in the zenith at Kendal, and elevated 53° from the north at Manchester, must be nearly one hundred miles high.

The results of this series of additional observations agreeing so nearly with that of the 29th of March, 1826, I am induced to believe that these luminous arches of the aurora which occasionally appear, stretching from east to west, are all of the same height, and that height about one hundred miles. What length the upright beams,—or to speak more properly, those parallel to the dipping needle,—may be, which are the ordinary forms of the aurora, we have not observations to determine. Whether those beams arise above the arches as from a base, or whether they descend below, as if appended to the arches, we cannot absolutely determine. It is remarkable that the arches and beams should rarely, if ever, be seen cognate or in juxta position, but always in parts of the heavens at a considerable distance from each other.

Manchester,

March 18, 1828.

POSTSCRIPT.

Query, Are the parallel bands usually about 20 degrees asunder? If so, their distance from each other will be about thirty-six miles.

XIV. *A comparison of the changes of magnetic intensity throughout the day in the dipping and horizontal needles, at Treurenburgh Bay in Spitsbergen.*
By CAPTAIN HENRY FOSTER, R.N. F.R.S.

Read May 8, 1828.

THE few observations I had an opportunity of making at Port Bowen in 1825, on the diurnal changes of intensity shown by the dipping and horizontal needles, first suggested the idea of a daily rotatory motion of the general polarizing axis of the earth, as the cause, not only of the diurnal changes of intensity, but also of the diurnal oscillations of the horizontal needle throughout the world. And the circumstance, of the times of the maximum and minimum effect of these phenomena, occurring generally when the sun bore north, south, east, and west by compass, indicated his agency in producing this motion of the pole.

The entire confirmation of an hypothesis so important in the theory of terrestrial magnetism, requires the evidence of varied and extensive observation; and as my professional pursuits have recently led me to revisit those regions best calculated for the experiments, I have thought a continuation of them under favourable circumstances, might prove an useful auxiliary to those already honoured with a place in the Philosophical Transactions for 1826.

The observations which I have now the honour to present to the Royal Society, were performed in a manner somewhat different from those alluded to at Port Bowen, which were made with one needle only, first as a dipping needle, and then suspended horizontally. Whereas in this case, two needles were employed, each in its respective capacity; an arrangement far more convenient in practice, and equally satisfactory as to the object I had in view; which was simply to ascertain, whether or not a corresponding change of intensity exhibited itself in both needles, whether each was differently affected thereby, or whether such change belonged to the horizontal needle alone.

The expression for the intensity of the dipping needle, is $I = 2A \sqrt{\frac{1}{4 - 3 \sin^2 \delta}}$, and for the horizontal needle, $I = 2A \sqrt{\frac{1}{3 + \sec^2 \delta}}$, δ denoting the dip, A a general co-efficient. Consequently, if the change took place in the co-efficient A only, that is in the general magnetic intensity, both needles would be proportionally affected, and in the same manner; but if the change were in the dip only, then the two needles would be differently affected; the dipping needle, as $\sqrt{\frac{1}{4 - 3 \sin^2 \delta}}$, and the horizontal needle, as $\sqrt{\frac{1}{3 + \sec^2 \delta}}$, so that one would increase in intensity and attain its maximum, while the other would decrease and attain its minimum, and *vice versa*.

If both δ and A be variable, the relation between the simultaneous intensities of the two needles would remain the same as if δ only changed, because it is common to both needles; but the comparison of the same needles, at different times of the day, would be considerably modified by such a change in the value of A , and which would appear to be the case both from the present observations and those at Port Bowen. For example, if A was a maximum when the dip was the greatest, and consequently when the horizontal intensity, from considerations of dip only, was the least, the one effect would in some measure counteract the other on the horizontal needle; whereas the dipping needle would have its intensity increased from both these causes operating at the same time, and contrariwise if at the moment of least dip A should be at its minimum. It is, however, by no means my intention at present to enter upon this intricate inquiry; my object being to examine whether the simultaneous changes in the intensities of the two needles are of a character to indicate a change of dip as one at least of the causes, or whether the dip remains constant, and the change is due to that of intensity alone. As far as this question is concerned, the results are certainly satisfactory; for on comparing the intensities of the two needles given in Table II. it will be found that the intensity of one needle was generally the greatest when the other was the least, and the contrary. That a change actually takes place in the general intensity of the earth's magnetism, is as an hypothesis very reasonable; still, however, it is but an hypothesis, and as such I shall not insist upon it in this place, notwithstanding the circumstantial evidence furnished by these observations, but leave

it for more extended experiments to decide, and proceed at once to a detail of those observations from which the former deduction is made.

With respect to the instruments, they differed so little from those employed at Port Bowen, which have already been described, that it is unnecessary to say more than that the dipping needle was one belonging to the Board of Longitude, and made by Dollond; the needle used was in form a parallelepipedon, 6 inches long, 0.4 broad, and 0.05 thick, and that its magnetism was not interfered with while it was in my possession. The same may be observed of the horizontal needle, which was one of the same form and weight as the above.

The experiments were commenced upon the 30th of July, and continued to the 9th of August by myself only; and they were so arranged, that in the course of two days an observation was made every hour of the twenty-four, but part of them in one day, and part of them in the other, as shown in the Table.

Previous to the commencement of the observations, the silk thread (eleven inches long) which was employed for suspending the horizontal needle, was divested as far as could be of torsion, by suspending a brass needle of like form and of equal weight with the one above described; it was then replaced by the magnetized needle itself, the centre of which was brought directly over the centre of a graduated circle, by means of foot-screws attached to a board on which the apparatus stood. The needle being thus freely suspended, it was drawn out of the magnetic meridian somewhat more than 40 degrees, by a contrivance for that purpose; but its oscillations were not noticed until the arc had decreased to 40 degrees, when the observations were commenced on the times of performing ten vibrations successively, until two hundred were completed; the terminal arc and temperature of the instrument were then registered, and in this manner all the results given in the following Table were obtained.

The vibrations on the dipping needle were taken as follows: viz. one hundred with the face of the instrument East, previous to those on the horizontal needle as above described; and another hundred after the latter, with the face West; so that the mean time of observation for both needles was nearly the same, as will be seen by referring to Table I., relative to which, however, it should be observed, that although two hundred vibrations were taken by each needle, the time of performing one hundred only is recorded in the Table, as also the mean arcs of vibration.

TABLE I.

Containing the Observations on the Diurnal Changes of Intensity in the Dipping and Horizontal Needles at Treurenburgh Bay in Spitsbergen, in the Months of July and August 1827.

Dates.	Dipping Needle.				Horizontal Needle.			
	Hour.	Time of performing 100 Vib ^{ns} .	Mean Arc.	Temp. FAHR.	Hour.	Time of performing 100 Vib ^{ns} .	Mean Arc.	Temp. FAHR.
July 30, A.M.	h m	s	°	°	h m	s	°	°
	2 4	291.6	23.7	35	1 56	619.0	26.2	34½
	3 4	291.3	24	34½	2 56	620.0	24	34½
	3 58	291.2	23.8	34	3 50	620.9	23.7	35
	4 57	291.4	23.8	34	4 48	619.0	24.5	34
	6 1	292.1	23.5	35½	5 52	618.7	25	35
	7 0	291.9	23.2	35½	6 56	619.4	26	36
	7 47	292.2	22.8	36	7 46	619.4	26	35
	5 35	291.2	21.3	36½	5 36	613.6	25.5	36½
	P.M. 6 3	291.2	21.2	35½	6 22	616.6	25	36½
	8 14	291.9	22.2	35½	8 19	615.6	25	36½
	8 56	291.7	22.2	35½	9 00	615.8	26	36½
	9 38	291.6	21.7	36	9 39	615.0	26	36
	10 26	291.6	21.7	36	10 37	615.1	25	36
	11 16	291.4	21.3	35½	11 25	621.1	26	36
	12 02	291.8	21.3	35	12 00	620.4	25	37
July 31, A.M.	9 17	292.2	21.7	40.2	9 23	618.6	25½	41½
	10 18	291.8	21.2	41.2	10 23	616.1	25½	43
	11 20	292.4	22	41	11 25	612.5	25½	41
	P.M. 0 18	292.6	22.2	40½	0 22	610.0	26	42
	0 57	292.6	22	40	0 54	611.0	25½	42
	1 56	292.8	22.3	40	1 54	613.2	26	42
	2 56	293.0	22.5	39½	2 54	619.1	26	40½
	3 52	292.5	22.2	39½	3 51	622.0	26	40
Aug. 1, A.M.	0 55	291.7	22.2	38	0 50	619.8	26	39
	1 55	292.0	22.1	38	1 55	618.6	25½	39
	2 54	291.6	21.9	38½	2 50	617.7	26	39
	3 52	291.6	22.2	39	3 51	618.3	25½	40
	4 53	292.3	22.3	39½	4 50	619.5	25½	40½
	5 50	292.4	22.1	40	5 47	618.9	25½	41
	6 57	292.3	22.2	41½	6 54	619.5	26	42
	7 41	292.4	21.8	41½	7 42	619.9	25½	43
	5 36	292.9	22	48½	5 37	617.0	25½	50½
	6 57	292.8	22½	47½	6 56	616.8	25½	46½
	7 57	292.6	22½	47½	7 58	617.4	25½	50
	9 1	292.6	22.3	47½	8 56	617.9	26	52
	9 55	292.3	22.2	44	9 50	617.1	25½	45
	10 46	292.3	22.3	42½	10 47	617.7	25½	44
	11 52	292.7	20.7	41½	11 48	617.5	25½	43

TABLE I. (Continued).

Dates.	Dipping Needle.				Horizontal Needle.			
	Hour.	Time of performing 100 Vib ^{ns} .	Mean Arc.	Temp. Fahr.	Hour.	Time of performing 100 Vib ^{ns} .	Mean Arc.	Temp. Fahr.
Aug. 2, A.M.	h m s				h m s			
	9 41	293.7	22.3	62.2	9 42	620.2	25½	64
	10 41	293.4	22.1	59.2	10 41	621.2	25½	59½
	11 39	293.5	22.1	58.2	11 41	619.0	25½	59
	0 50	293.8	22.2	57½	0 48	616.6	26	58
	1 53	293.3	21.9	56½	1 48	618.8	25½	56
	2 49	293.7	21.7	53.7	2 46	617.8	25½	55½
	3 37	293.6	21.1	53	3 40	619.0	25½	53
Aug. 3, A.M.	0 44	291.7	21.6	47	0 45	619.8	25	52
	1 46	292.2	21.5	47	1 47	618.8	25	51½
	2 45	292.0	21.3	48½	2 46	619.9	25	52
	3 41	291.7	21.5	50	3 43	619.7	25	55
	4 42	292.2	21.8	51	4 45	616.9	25	57
	5 45	293.0	22.1	52½	5 45	617.7	25	57
	6 43	293.4	22.1	54½	6 44	625.4	25	59
	7 46	293.4	21.9	56½	7 49	620.0	25	61
	8 41	293.7	22.7	57½	8 42	620.2	25	58
	8 2	293.6	22.7	53	8	619.7	25	54
	8 44	293.1	22.5	51½	8 46	619.8	25	52
	9 46	293.4	22.5	49½	9 47	621.1	25½	51
	10 46	293.1	22.2	49½	10 48	618.2	25	51
	11 41	292.2	22.1	49	11 43	617.3	25	53
Aug. 4, A.M.	9 12	293.7	22.3	56½	9 16	620.2	26	61
	9 57	293.9	22.7	59½	9 55	618.5	25½	63
	10 44	293.3	22.5	60	10 45	619.2	26	63
	11 37	292.6	22.1	59½	11 39	619.5	26	62
	0 40	292.8	22	60	0 41	619.2	26	63
	1 59	292.8	21.7	62½	1 55	619.0	25½	63
	2 42	293.6	22	61½	2 44	619.0	25½	64
	3 36	293.8	22.1	63½	3 38	618.4	25½	65
Aug. 5, A.M.	1 00	291.9	22	51	0 57	620.0	23	52
	1 43	291.5	22	51	1 44	620.8	25	52½
	2 45	291.9	22.1	51	2 47	619.5	24½	52
	3 41	291.5	21.7	50	3 42	618.5	24	51
	4 44	292.1	22.5	50	4 45	620.2	24	51
	5 41	291.4	21.8	47½	5 43	618.4	23½	49½
	6 43	291.8	22.1	49	6 45	616.4	24	51
	7 35	292.5	22.3	53	7 36	621.1	24	52½
	8 44	293.6	22.2	49½	8 46	619.2	26	49½
	6 35	293.1	22.7	49½	6 36	617.7	25	50
	8 1	291.6	22.4	47	7 57	620.7	25.5	47½
	8 46	292.1	22.2	47½	8 45	619.6	25.5	48
	9 45	291.8	22	47	9 46	619.6	26.5	46½
	10 46	292.5	22.3	47	10 47	618.3	25.5	47½
	11 43	292.0	22.2	46½	11 47	618.1	25	47

TABLE I. (Continued.)

Dates.	Dipping Needle.				Horizontal Needle.			
	Hour.	Time of performing 100 Vib ^{ts} .	Mean Arc.	Temp. Fahr.	Hour.	Time of performing 100 Vib ^{ts} .	Mean Arc.	Temp. Fahr.
Aug. 6, A.M.	h m	s	°	°	h m	s	°	°
	9 20	293.0	23.3	53½	9 26	616.6	26	54
	10 00	292.7	23.3	54½	9 57	615.3	25½	54½
	10 42	292.4	23	55½	10 44	614.0	26	55
	11 41	293.2	22.6	54½	11 43	613.2	25.5	50
	P.M. 0 51	292.6	22.4	56½	0 49	614.7	25.5	57
	1 40	293.0	22.9	55	1 41	615.3	26	56
	2 51	292.9	23	55½	2 49	616.8	26	58
Aug. 7, A.M.	3 37	293.2	23.5	58½	3 38	615.3	25.5	57
	0 46	292.7	22.6	47	0 47	618.9	24.5	49
	1 42	291.8	22	48½	1 43	620.3	25	51
	2 44	292.5	22.5	45½	2 47	619.6	25	46½
	3 38	292.1	22.9	45	3 38	620.9	25	46
	4 41	292.2	23	44½	4 42	620.0	25	45
	5 36	292.2	23	46	5 38	620.1	25.5	45
	6 41	292.1	23	43½	6 42	620.3	25.5	45
P.M.	7 38	292.7	23.2	42½	7 39	620.2	25.5	44
	5 37	292.1	23.6	41½	5 38	619.1	25.5	43
	6 42	292.1	23.5	40½	6 44	616.7	25.5	42½
	8 13	291.8	23.7	41	8 18	618.0	25.5	41½
	9 00	292.7	23.5	41½	8 57	618.0	25.5	41½
	9 42	292.1	23.6	40½	9 43	618.1	25.5	41
	10 41	292.0	23.5	40	10 43	617.7	26	41
	11 44	291.2	23.5	40½	11 45	623.1	25.5	40½
Aug. 8, A.M.	9 18	291.6	23.5	47½	9 23	618.2	25.5	49
	10 6	292.6	23.4	51	10 14	619.9	26	53
	10 56	292.7	23.2	55½	10 56	618.5	26	56
	11 39	293.1	23.1	56	11 40	618.0	26	57
	P.M. 0 38	293.5	23.1	57½	0 39	617.8	26	58½
	1 41	293.7	23.6	59	1 43	617.6	25.5	59
	2 39	293.8	23.3	58	2 41	622.1	25.5	60
	3 36	293.3	23.5	58½	3 38	619.9	26	60
Aug. 9, A.M.	0 58	291.6	23.1	50	0 55	624.0	25	53½
	1 44	291.7	23.2	49½	1 45	622.5	25	52½
	2 37	291.6	23.5	50	2 39	623.2	25	53
	3 38	292.0	22.5	50½	3 38	625.3	25	54
	4 40	292.2	22.7	50	4 41	623.1	24.5	55
	5 55	292.1	22.3	52½	5 53	622.1	21	54
	6 40	291.6	21.8	54	6 39	621.7	22.5	60
	7 35	293.3	22.3	57	7 36	623.0	25.5	61
P.M.	5 40	293.0	23.1	58	5 41	621.6	25.5	58
	6 40	293.3	23	58½	6 42	622.6	26	58
	8 17	293.0	23.2	56	8 12	621.9	25.5	56½
	8 52	292.7	23.2	54½	8 50	621.5	25.5	55½
	9 42	292.3	22.9	53½	9 43	621.8	25	55
	10 41	291.9	22.7	54	10 42	619.8	25.5	54
	11 41	292.2	22.8	50½	11 43	619.5	25	53

In order to bring more clearly into view the results of the foregoing observations, the following Table has been formed, by adding together all the *times* each needle took to perform one hundred oscillations at the respective hours of observation on the several days, and converting these times into proportional intensities. In the third and sixth columns are inserted the numbers expressive of intensities, which have been obtained, by squaring the reciprocal of the times, and multiplying those squares by 100,000, to render them all integral.

TABLE II.

Dipping Needle.			Horizontal Needle.		
Hour.	Time in seconds of performing 100 oscillations.	Proportional Intensity.	Hour.	Time in seconds of performing 100 oscillations.	Proportional Intensity.
A.M. ^{h m} 0 52	291.9	1173	A.M. ^{h m} 0 51	620.5	2597
1 49	291.8	1174	1 46	620.0	2601
2 48	291.8	1174	2 48	620.0	2601
3 44	291.7	1175	3 44	620.6	2596
4 46	292.1	1172	4 45	619.8	2603
5 48	292.2	1171	5 46	619.3	2607
6 47	292.2	1171	6 46	620.4	2598
7 40	292.7	1167	7 41	620.6	2596
9 16	292.6	1168	9 22	618.3	2616
10 00	292.9	1166	10 2	618.0	2618
10 52	292.8	1167	10 54	617.1	2626
11 47	293.0	1165	11 49	615.9	2636
P.M. 0 47	293.1	1164	P.M. 0 46	615.9	2636
1 50	293.1	1164	1 48	616.8	2629
2 47	293.4	1161	2 47	618.9	2611
3 40	293.3	1162	3 41	618.9	2611
5 43	292.3	1171	5 47	617.8	2620
6 43	293.0	1165	6 44	618.8	2612
8 7	292.4	1170	8 7	618.9	2611
8 53	292.5	1169	8 52	618.8	2612
9 51	292.1	1172	9 52	618.2	2617
10 49	292.2	1171	10 52	618.8	2612
11 47	292.0	1173	11 48	619.3	2607

On looking over this Table it will be seen, that when an increased intensity obtained in the dipping needle, a corresponding diminution generally exhibited itself in the horizontal needle, and vice versa. But to compare this Table with the results deduced from my former experiments, and with the hypothesis of a rotation of the general polarizing axis of the earth about its mean position

as a centre, it will be best to express the intensities in terms of the magnetic latitude; viz.

$$* I = A \sqrt{3 \sin^2 \lambda + 1} = \text{Intensity dipping needle,}$$

$$I = A \cos \lambda = \text{Intensity horizontal needle:}$$

where λ is the magnetic latitude. If we assume 81° as the mean dip at Treurenburgh Bay, and since

$$\text{Tang. } \delta = 2 \text{ tang. } \lambda,$$

or $\text{tang. } \lambda = \frac{1}{2} \text{ tang. } 81^\circ$, we have $\lambda = 72^\circ 30'$ the mean magnetic latitude. Let x be the radius of the circle of rotation assumed in the hypothesis; then the extreme latitudes will be $72\frac{1}{2}^\circ + x$ and $72\frac{1}{2}^\circ - x$, and the intensities varying in their greatest extreme, as 2596 to 2636; we have to find x such, that

$$\cos (72\frac{1}{2}^\circ + x) : \cos (72\frac{1}{2}^\circ - x) :: 2596 : 2636.$$

This gives $x = 8$ minutes very nearly; whereas in my former paper I suppose this radius not to exceed $2\frac{1}{2}$ minutes. In that case, however, the deduction was made from the mean results of several months' observations, commencing with January, when the effect is the least; whereas this is drawn from the extreme results of eleven days only, and at that season when the effects are the greatest.

Assuming the above radius of rotation, viz. 8 minutes, it is easily ascertained that the corresponding maximum change in the daily variation ought to be 54 minutes; whereas it appears by the Table published in the Appendix to Captain PARRY'S Narrative of his Attempt to reach the North Pole of the Earth, to have amounted to $1^\circ 32'$ from the mean of eleven days' observations.

The $2\frac{1}{2}$ minutes assumed as the radius of rotation of the magnetic pole, from the mean of the Port Bowen experiments, is certainly too small to answer even to the mean results: but if we take the mean results as there obtained for the month of May, which was the greatest observed, it will be found that the radius of rotation would require to be taken at 8 minutes, the very quantity above determined. It will, however, be seen by referring to the paper in which the former experiments and observations are recorded, that the $2\frac{1}{2}$ minutes assumed for the radius was stated as a quantity altogether conjectural, no attempt being made to establish it by calculation. At all events it will be

* Barlow's Essay on Magnetic Attractions, page 197, 2nd edition.

necessary to consider this radius as changing very considerably as the sun advances to the north.

The change of intensity in the dipping needle as depending on the change of dip or of the magnetic latitude, would be only as 3726 to 3732, whereas it is found to amount to $\frac{1}{3}$ rd part of the whole. This therefore seems to indicate a change in the general co-efficient A, and that this is greatest when the dip is greatest, and least when the dip is least.

We might be able to separate these two counteracting effects on the horizontal needle; but it would probably be considered too speculative in the present stage of this inquiry. All therefore that I shall consider as demonstrated by these experiments is, that the cause of the daily change in the horizontal intensity is principally due to a change of dip, as I found to be the case at Port Bowen, and that the times of the day when these changes are the greatest and least, point clearly to the sun as the primary agent in the production of them; and that this agency is such as to produce a constant inflection of the pole towards the sun during the twenty-four hours: this is, I think, clearly established as far as comparison has yet been made, and I hope soon to be able to submit this inquiry to the test of experiments under circumstances so different in every respect from these and the former, that they cannot fail of either confirming or contradicting the hypothesis in question.

London,
March 4th, 1828.

HENRY FOSTER.

XV. *Experiments relative to the effect of temperature on the refractive index and dispersive power of expansible fluids, and on the influence of these changes in a telescope with a fluid lens.* By PETER BARLOW, Esq. F.R.S. &c.

Read May 15, 1828.

IN a paper I had the honour to present to the Royal Society in January last, relative to the construction of achromatic telescopes with fluid lenses, I have stated that between the temperatures of 31° and 84° I had not been able to detect any very sensible change in the index denoting the focal length of the telescope: these observations however being made at intervals of some months, I was doubtful whether there might not be some minute variation which had escaped my notice; and I have since, by means of temperature artificially produced, ascertained that there is a certain small change, and the amount of that change, which is $\frac{1}{10000}$ ths of an inch in the length of the telescope employed, between each of these extremes and the mean temperature of 57° . That is, the eye-piece of the telescope and the fluid lens being fixed, as was the case in this instrument, the plate lens required an adjustment of 0.134 of an inch, between the temperature of 57° and each of the above extremes, to produce the brightest and most perfect image.

Before I proceed, however, to detail the results of my inquiry on these subjects, it will be proper to define a few terms which appear in one or two instances to have been misunderstood.

1. The length or focal length of the telescope, is the distance from the front lens to the focus.

2. The fluid focus or fluid focal length, is the distance from the fluid lens to the focus.

3. The focal power of the telescope, or the equivalent focal length, is the focal length of a telescope of the usual construction, which gives the same convergency to the rays or the same sized image as the telescope in question.

In what follows :

l will denote the length of the telescope.

f the focal length of the plate lens.

f' the focal length of the fluid lens.

f'' the fluid focal length.

f''' the equivalent focal length.

d the distance of the lenses.

Under the particular form of construction to which we are now referring, f and f''' remain fixed or constant, but the rest are variable under different temperatures, in consequence of the effect which temperature produces in the value of f' .

If we knew the change in the value of f' , or in the refractive index of the fluid under different temperatures, we might proceed immediately to compute its effects on the focal power of the telescope; but as this may be considered doubtful, I have endeavoured to determine the effect on the power of the telescope by direct observations, and have thence computed the corresponding change in the refractive index of the fluid.

In order to determine the change in the position of the front lens due to a certain range of the thermometer, I placed the telescope in a small room about twelve feet square in my garden, and having adjusted it very carefully to a dial-plate of a watch, at the distance of 150 feet, when the thermometer was at 40° , I had a fire lighted, the room shut up, and the temperature gradually raised to 75° , re-adjusting and registering the focus for every change of 5° .

As, however, the intermediate changes were very small, it will be sufficient to state, that between the two extremes, viz. 40° and 75° , the whole change was 0.177 of an inch; and hence, supposing the change uniform for equal variations of temperature, we find for the difference between the mean temperature of 57° and each extreme before mentioned, viz. 31° and 84° , an alteration in the length of the telescope of .134 of an inch, as stated in the beginning of this paper.

In the instrument on which these observations were made, the following are the values of the different quantities at the mean temperature 57° ; viz. $f = 32.5$,

$$f' = 32.65, \quad f'' = 40.5, \quad l = 54.92, \quad d = 14.42, \quad f''' = \frac{ff''}{f-d} = 72.8.$$

And since by the observations above referred to, the value of d varied 0.134 of an inch between the mean temperature and each of the extremes, we have in one case $d = 14.554$, and in the other $d = 14.286$. Whence the focal power of the telescope was

$$\text{at } 31^\circ, f''' = \frac{ff''}{f-d} = 73.34$$

$$\text{at } 84^\circ, f''' = \frac{ff''}{f-d} = 72.28$$

So that the instrument being adjusted at the mean temperature 57° , and fitted with a micrometer, it will require a correction of about $\frac{1}{3600}$ th part of the angular measure for every change of 1° in the thermometer; that is, a 60th part of a second for every minute in the angle, a quantity too small to require any notice, except in cases of extreme delicacy.

In order to find the actual change in the focus of the fluid lens which rendered the foregoing adjustments of the plate lens necessary, we have

$$\frac{1}{f-d} - \frac{1}{f'} = \frac{1}{f''} \quad \text{or} \quad \frac{1}{f-d} - \frac{1}{f''} = \frac{1}{f'}$$

In this expression, $f'' = 40.5$, $f-d$ at $31^\circ = 17.946$

$$f-d \text{ at } 57^\circ = 18.080$$

$$f-d \text{ at } 84^\circ = 18.214$$

And substituting these values successively for $f-d$ in the above expression, we find

$$f' \text{ at } 31^\circ = 32.222$$

$$f' \text{ at } 57^\circ = 32.650$$

$$f' \text{ at } 84^\circ = 33.090$$

And since it has been shown, Phil. Trans. 1827, Art. XV. that $\frac{f-d}{ff'}$ is dispersive ratio, we have at 31° dispersion = .3067

$$57^\circ \text{ dispersion} = .3075$$

$$84^\circ \text{ dispersion} = .3084$$

a difference sufficiently small to baffle the most acute and experienced eye. The change therefore in the power and colour of the telescope is so small, and the correction due to it (in any case where such correction is thought necessary) so easily made, that an instrument on this construction may I trust be considered just as applicable to all the nice purposes of modern astronomy, as one of the usual refractors of the same power.

The very inconsiderable change in the focal power of the telescope led me to conclude, in the early part of my experiments, that no optical change took place in the fluid between the above limits, or at least that the change was extremely small. It appears however from the preceding experiments and investigation, that the permanency of the telescopic effect is attributable to the peculiar construction of the instrument, and that the change in the refractive index of the fluid is much more considerable than I had imagined; for we have seen that the focal length of the fluid lens was at $31^{\circ} = 32.22$

$$\text{at } 57^{\circ} = 32.65$$

$$\text{at } 84^{\circ} = 33.03$$

And since the focal length is *cæteris paribus* inversely as the index, and the index at 57° being 0.634, we find $32.22 : 32.65 :: 0.634 : 0.642$

$$33.09 : 32.65 :: 0.634 : 0.626$$

Hence the mean index of the sulphuret of carbon is at $31^{\circ} = 0.642$

$$\text{at } 57^{\circ} = 0.634$$

$$\text{at } 84^{\circ} = 0.625$$

That is, with a variation of temperature of 53° ; the change of index amounts to $\frac{.017}{.634} = \frac{1}{37}$ th part nearly of the whole index at 57° .

Which, supposing the change to be uniformly proportional in greater thermometrical ranges, gives a change in the refractive index of nearly $\frac{1}{10}$ th between 32° and 212° . Now it has been stated on the result of experiment (Dr. Ure's Chemical Dictionary), that the expansion of sulphuret of carbon amounts to $\frac{1}{4}$ th between the above limits. We have therefore strong reasons to conclude that in this, and all other expansible fluids, the index of refraction varies directly as the density; the trifling difference in the two results being attributable, in all probability, to slight errors of observation in one or other of the two processes, so different from each other, from which these results are deduced.

With respect to the dispersive ratio, it is probably the same at all temperatures; for, supposing $1 : 1 + a$ $1 : 1 + a'$, $1 : 1 + a''$ to be the ratio of the sines of incidence and refraction of the extreme and mean rays of the spectrum at any given temperature, the dispersive power is expressed by $\frac{a-a''}{a'}$. And as we have seen that the mean index a' varies as the density of the fluid, we

have strong reason to suppose that a and a'' vary also in the same proportion; and if so, the dispersive power will of course remain constant; and this deduction is verified, as far as the eye can judge of colour in the telescope, by the preceding experiments, which certainly indicated no perceptible change in the colour of the image. This, however, is a subject I intend to examine more particularly when my large telescope is completed.

It may be proper to observe, that the form of the instrument here employed differs a little from that described in my former paper: in the latter, the plate lens is a fixture, and the adjustment is made by a slight motion of the fluid lens. In this I can move either lens at pleasure; and I have chosen to fix the fluid, and to adjust the plate lens, merely for the sake of simplifying the investigation.

THE HISTORY OF THE UNITED STATES OF AMERICA

FROM THE FIRST SETTLEMENTS TO THE PRESENT TIME
BY JAMES M. SMITH
VOLUME I
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XVI. *On some circumstances relating to the economy of bees.* By THOMAS ANDREW KNIGHT, Esq. F.R.S. *President of the Horticultural Society.*

Read May 22, 1828.

IN a paper which I had the honour to address to the Royal Society about twenty years ago (in the year 1807) upon the Economy of Bees, I stated, that having adapted cavities in hollow trees for the reception of swarms of those insects, I had observed that several days previous to the arrival of a swarm, a considerable number of bees were constantly employed in examining the state of the tree, and particularly of every dead knot above the cavity which appeared likely to admit water into it. At that period it appeared to me rather extraordinary, that animals so industrious as bees, and so much disposed to make the best use of their time, should, at that important season of the year, waste so much of it in apparently useless repetitions of the same act: for I, at that time, supposed that on different days, and at different periods of the same day, I saw only the same individuals. But in a case which at a subsequent period came under my observation, where the cavity into which the bees apparently proposed to enter, was not more than a quarter of a mile distant from the hive whence a swarm were prepared to emigrate, I witnessed a very rapid change of the individuals who visited their future contemplated habitation; and the number which in the course of three days entered it, appeared to me to be fully equal to constitute a very large swarm: and upon the evidence of these and other facts, which I shall proceed to state, I am much disposed to infer, that not a single labouring bee ever emigrates in a swarm without having seen the future proposed habitation of that swarm. That the queen bee has also always seen her future habitation, I am also much inclined to believe, as she is well known to absent herself from the hive some time previously to the emigration of a swarm: though her object may be to meet a male of another hive; for I much doubt whether she ever receives the embraces of a brother. The results of some of HUBER's experiments are very favourable

to this conclusion, as is the otherwise excessive number of male bees, and in both the animal and vegetable world nature has taken very ample means of facilitating what the breeders of improved varieties of domesticated animals call cross breeding.

I have also been led by the following facts to believe, that not only the future permanent habitation of each swarm, but the place where they temporarily settle, apparently to collect their numbers, soon after they quit their hive, is known also to each individual. Different families of domesticated animals of every species present some peculiarities of disposition and habit; and the swarms of the family of bees, which were the subject of my experiments, showed, I think, more than an ordinary disposition to unite, by two apparently joining the same queen. My attention was consequently attracted to the circumstances which preceded such unions.

The simultaneous movements and agitation of two hives, had during several days led me to expect that a junction of their swarms was contemplated; and the two ultimately issued out almost at the same moment, and instantly united, as I had concluded they would. The weather was excessively hot; and I put them into a hive which was scarcely large enough to hold them, affording them no further shelter from the sun than I thought just sufficient to prevent the melting of their combs. This occurred upon the first day of June, and in the morning of the twenty-third a very large swarm emigrated. There was in this, I believe, nothing very extraordinary or peculiar, except the excessive expedition apparently employed in raising a second queen.

In the following year two other hives presented similar indications that their swarms would unite; and being anxious to ascertain whether such unions were accidental, or the consequence of previous arrangements, I paid very close attention to their proceedings, and the following singular circumstances came under my observation. After both hives had given frequent indications that a swarm was ready to issue from each of them, one swarm only rose, and that, after hovering in the air during a much longer time than ordinary, settled upon, and around, a bush about twenty-five yards distant from the hive, whence they had issued: but instead of collecting together into a compact mass, as they usually do, they remained thinly dispersed, scarcely two being any where in contact with each other. In this state they continued nearly half an hour

motionless and apparently discontented and sulky; and they then gradually began to rise and return home, not apparently in obedience to any command or signal; for they did not rise more abundantly at any one point of time than at another, but each individual seemed to go when tired of waiting.

The next morning a swarm issued from the other hive, and proceeded to the bush upon and around which the other swarm had settled on the preceding day, collecting themselves into a mass as they usually do when their queen is present. This was precisely what I had anticipated, but I was much disappointed that no movement or agitation took place in the other hive. Within a very few minutes, however, and very soon after the swarm above mentioned had fully settled, a very large number of bees suddenly rushed from the hive to which the swarm had returned on the preceding day, and proceeded so directly to the swarm which had just settled, that their course was marked through its whole extent by a perfectly visible dark and narrow line, and they united themselves, without hovering a single instant, to the other swarm. These circumstances conjointly with others which I have stated in my former communication upon this subject, satisfied me that these unions are generally, if not always, the result of previous and perfectly well understood arrangements, though it is not easy to conjecture how such arrangements can be made.

I shall proceed to state a few circumstances which appear to throw light upon some of the phenomena observable in the mode of breeding of bees. It has long been known that these animals possess the power of raising a queen bee from any recently deposited egg, which under ordinary circumstances would have produced a labouring bee; but whether this power extends to those eggs, which, when deposited in larger cells, afford male or drone bees, has not, I believe, been accurately ascertained. The following circumstances lead me to believe that sex is not given to the eggs of birds, or to the spawn of fishes or insects, at any very early period of their growth.

I selected early in winter four female birds of the common duck, which I kept apart from any male bird of that or any kindred species, till the period of their laying eggs approached. One was then killed, and the largest of its eggs was found to be three lines in diameter. A musk drake (*Anas moschata*) was then put into company with the three remaining ducks; and from these I obtained a numerous offspring, six out of seven of which proved to be males,

as the result of similar previous experiments (but in which the male of another species had been introduced at a period when the growth of the eggs was less advanced,) had led me to expect. I repeated the experiment often, and always with nearly the same result, a large majority of male birds being uniformly produced; and hence I conclude that the eggs of birds in early periods of their growth are without sex.

I have never possessed means of obtaining mule fishes; but one kind of fish, which I think is obviously a mule, is found in many rivers where the common river-trout abounds, and where a solitary salmon is sometimes seen. These formerly existed, in some seasons, in considerable numbers, in the river which passes near my residence; but since salmon have become scarce, they have wholly disappeared. I had formerly opportunities of examining a large number of them, without having ever found a single female. I have subsequently found them in large numbers in small mountain rivulets in Wales, below, but never above, the lowest Cataract. They are readily distinguished from the young salmon, by their form being intermediate between that of a trout and of a salmon; by their being all, or nearly all, males; and by their remaining through the summer and autumn in the rivers, long after the young salmon have descended to the sea: they leave the fresh water with the first winter floods, and I believe are not known ever to return. In the North of England they are distinguished by the name of wrackriders, and by that of samlets in some other parts. If these be mules, as I do not entertain any doubt that they are, the spawn of fishes must be without sex when it is deposited by the female; and I am much disposed to entertain the same opinion respecting the spawn (for it is more properly spawn than eggs) of bees.

I have frequently witnessed some somewhat analogous circumstances in the vegetable world, respecting the sexes of the blossoms of plants; and I can at any time succeed in causing several kinds of monoecious plants to produce solely male or solely female blossoms. If heat be, comparatively with the quantity of light which the plant receives, excessive, male flowers only appear; but if light be in excess, female flowers alone will be produced:—the experiments necessary must of course be made with skill and accuracy.

In a former communication to the Royal Society "Upon the comparative influence of the male and female parent upon the character of the offspring,"

I have inferred, from facts there stated, that the sex of the offspring of some species of animals is given by the female parent. Subsequent experience and observation have strengthened my belief in the truth of this inference: but I believe the power of the female parent to be rather strongly influential than positive, and that external causes operate, which (I have some reason to suspect) are not in all cases wholly beyond the reach of human controul.

THOMAS ANDREW KNIGHT.

London,
May 20th, 1828.

I have the honor to acknowledge the receipt of your letter of the 10th inst. in relation to the proposed extension of the term of office of the Board of Directors. I have conferred with the Board and they have decided to extend the term of office of the Board for another year, to the 31st day of December, 1901.

I have also the honor to acknowledge the receipt of your letter of the 15th inst. in relation to the proposed extension of the term of office of the Board of Directors. I have conferred with the Board and they have decided to extend the term of office of the Board for another year, to the 31st day of December, 1901.

I have also the honor to acknowledge the receipt of your letter of the 20th inst. in relation to the proposed extension of the term of office of the Board of Directors. I have conferred with the Board and they have decided to extend the term of office of the Board for another year, to the 31st day of December, 1901.

I have also the honor to acknowledge the receipt of your letter of the 25th inst. in relation to the proposed extension of the term of office of the Board of Directors. I have conferred with the Board and they have decided to extend the term of office of the Board for another year, to the 31st day of December, 1901.

I have also the honor to acknowledge the receipt of your letter of the 30th inst. in relation to the proposed extension of the term of office of the Board of Directors. I have conferred with the Board and they have decided to extend the term of office of the Board for another year, to the 31st day of December, 1901.

I have also the honor to acknowledge the receipt of your letter of the 31st inst. in relation to the proposed extension of the term of office of the Board of Directors. I have conferred with the Board and they have decided to extend the term of office of the Board for another year, to the 31st day of December, 1901.

XVII. *On the laws of the deviation of magnetized needles towards iron.* By
SAMUEL HUNTER CHRISTIE, *Esq.* M.A. F.R.S. &c.

Read June 5, 1828.

THE deviations of a magnetized needle from its natural direction in the plane in which it is constrained to move, due to the action of masses of iron, may be referred to a very simple law, excepting in those cases where the length of the needle bears a very sensible ratio to the distance of the iron. The law is this: if we suppose that the centre of a magnetic particle in the direction of the terrestrial magnetic force, or the centre of a small magnetic needle freely suspended by its centre of gravity, coincides with the centre of the needle whose motion is restricted; that the iron attracts both poles of this particle, or freely suspended needle; and that the whole, or very nearly the whole action takes place on these poles;—then the position of the other needle, in the plane in which it is constrained to move, will be found by referring the freely suspended needle to that plane, by a plane perpendicular to the first. The truth of this being established by experiment, it is very clear that whatever may be the position of a mass of iron, the direction of the deviations of a horizontal or dipping needle due to its action, will be immediately indicated, and a sufficiently simple calculation will give the amount. Several years have elapsed since I first pointed out this law, showing at the same time, by a series of experiments, that the observed deviations are in conformity with it. I have since omitted no opportunity of submitting it to the test of experiment; and taking it as the basis of calculation, have always found, except indeed in cases, as I have before stated, of too great proximity of the disturbing body, that the results which I obtained approximated so closely to the observations, as to leave no doubt in my own mind of its correctness.

However, the truth of this law has latterly been called in question; and, in a paper published in the Transactions of last year, some experiments on horizontal needles, having their magnetism unequally distributed in the two

branches, are described, which are considered by the author as quite decisive of its fallacy*. It is not my intention to enter into an examination of what are there, erroneously I apprehend, considered as the effects that would result from this law, when the equal distribution of magnetism in the two branches of a needle has been disturbed. Immediately after having heard that paper read, I proceeded to ascertain some circumstances which appear to have been overlooked in these experiments, and then noted what, according to the law in question, ought to be the deviations of a needle having either of its branches "deteriorated" when in different positions with respect to an iron shell. Not being then in possession of the experimental results in that paper, I could not compare my conclusions with them; but on doing so when the Part of the Transactions was published, I found them perfectly to accord.

Whatever hypothesis we may adopt for the explanation of the phænomena observed with a needle having its magnetism thus disturbed, or to whatever laws we may apply these phænomena as tests, it is essential that we should know, not only to which end of the needle the disturbing cause has been applied, and its effect on the directive force, but likewise the effect produced on the distribution of the magnetism throughout the needle. It is particularly necessary that the positions of the points where the intensity of the magnetism is the greatest, and of that where the magnetism of contrary names, and on opposite sides balance each other, and which may be termed the magnetic centre of the needle, should be determined. As this had not been done in the experiments to which I have alluded, it became necessary to repeat those experiments, and to determine the positions of these points in the needles employed in that repetition. With this view, I cut three needles of precisely the same form and weight, from the same piece of steel, and applying them over each other, hardened the ends of the three at the same time. These were successively placed in the middle of a groove which two twelve-inch bar magnets exactly fitted, and were each magnetized in precisely the same manner, by passing the ends of the bar magnets, inclined always at the same angle, from centre to ends, the same number of times. One of these was left thus magnetized. The other two were successively placed on a thin board, and the marked

* Phil. Trans. 1827. p. 281.

end of one of the twelve-inch bar magnets was passed twice, on the opposite side of the board, from the centre to the marked end of one needle; and the unmarked end of the magnet, similarly, from the centre to the unmarked end of the other*.

Having determined in these needles the points of greatest intensity, which we may consider as the poles, and likewise the centres of their magnetism, I made observations with them precisely similar to those detailed in the paper already cited†. In the first instance, I proposed to determine the several values of a particular constant, by applying the hypothesis of a central action of the iron to the observations with the needle whose magnetism had been undisturbed, and to employ the mean value in calculating the effects which would arise, according to this hypothesis, from the disturbance of the magnetism in the other needles: but as these values, though not differing greatly from each other, had a pretty regular increase and decrease according to the azimuth of the shell, I considered that the length of the needle might, in this case, bear too great a proportion to the distance of the shell for the proposed law to be strictly applicable, although it might give results approximating to the experiments. The fact however, which is stated to have been ascertained by experiment, that the length of a needle has no sensible influence on the extent of its deviations‡, was opposed to this view; and I therefore compared with each other the values of the constant which would result from these observations, by applying to them a law considered to have been established by experiment: viz. "the tangent of the deviation is proportional to the rectangle of the cosine of the longitude, and the sine of the double latitude" of the needle's centre with reference to that of the shell§.

The values of the constant, thus deduced, differed so widely from each other, that I could have no expectation of obtaining from the mean value, results that should at all approximate to the experiments; and indeed, could entertain no

* These terms, marked end and unmarked end, I employ instead of north end and south end, or south pole and north pole, merely to avoid the ambiguity that might arise in some of the experiments if the name of the end of the magnet or needle had any reference to position.

† Phil. Trans. 1827. p. 284.

‡ Mr. Barlow's Magnetic Attractions, second edition, p. 59.

§ Ibid. p. 39.

doubt of the fallacy of this law, even as approximative. These circumstances led me to suspect the accuracy of the conclusion, that the length of a needle has no sensible influence upon its deviation; and I considered that it would be desirable to ascertain whether such were or were not the fact. This in a theoretical point of view is of some importance; since such a fact would be in direct opposition to the conclusions derived from the theory advocated in the paper in question, and so ably and elaborately developed in M. Poisson's *Memoirs on Magnetism*. M. Poisson says: "A la vérité, M. BARLOW annonce qu'ayant placé successivement dans le même point le milieu de l'aiguille de six pouces, et celui d'une petite aiguille d'un demi-pouce (d'un pouce et demi?) en longueur, il n'a pas observé de différence entre leur déviations; ce qui ferait penser que les deux corrections dont nous parlons, dont l'une a pour effet d'augmenter la déviation, et l'autre, de la diminuer, se seraient à peu près compensées. Mais nous avons lieu de croire que cette compensation a été très-imparfaite; car en calculant les déviations de l'aiguille, sans avoir égard à la double corrections due à sa longueur et à sa force magnétique, les différences que l'on trouve entre le calcul et l'expérience, sont trop grandes pour être attribuées en entier aux erreurs des observations*."

These considerations induced me to extend my experiments much beyond the limits which I had originally proposed to myself; and as these experiments were made with great care, and I may fairly state, without any consideration of what might or might not be in conformity with theoretical views, they will afford good tests which may be applied to any theory of magnetism, as well as to the laws with which I have compared them. Previously to detailing any of these experiments, or to entering upon any investigations founded upon the law which they so decidedly establish, I shall briefly notice the facts which I ascertained, and the general explanation they afford, in conformity with that law, of the experiments which are considered by their author to be decisive of its fallacy.

In the first instance, I ascertained that if any bar of steel, uniformly magnetized by the method of double touch, have this state of its magnetism disturbed, by drawing the end of a magnet from its centre to the end of the same

* *Mémoire sur la Théorie du Magnétisme*, p. 87.

name as that applied to it; that is, drawing the marked end of the magnet towards the marked end of the needle, or the unmarked end towards its unmarked end; then the pole at the end to which the magnet has been applied, or that which has been termed the "deteriorated branch" will approach the centre of the needle. In the other, or "undeteriorated branch," the pole will recede from the centre.

And the magnetic centre will invariably recede from the centre of figure towards the "undeteriorated end," or that to which the disturbing magnet has not been applied. The changes in the positions of these points, in consequence of the disturbance of the magnetism, is best illustrated by the following figures, in which I. II. represent two bar magnets, 8.92 inches in length, 0.16 inch in breadth, 0.09 inch in thickness.

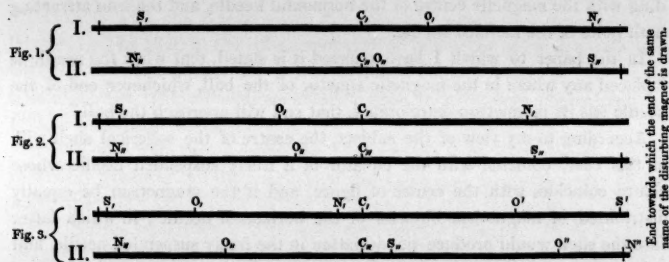


Fig. 1. represents the positions of the poles and magnetic centres after the bars had been magnetized by double touch: N_p , N_u the north poles; S_p , S_u the south poles; C_p , C_u the centres of the bars; O_p , O_u their magnetic centres.

$$C_p S_p = 3.74 \text{ inches; } C_p O_p = 1.14 \text{ inch; } C_u N_u = 3.97 \text{ inches;}$$

$$C_u N_u = 3.84 \text{ inches; } C_u O_u = 0.24 \text{ inch; } C_u S_u = 3.86 \text{ inches.}$$

Fig. 2. represents the positions of these points when the magnetism had been disturbed, by drawing the end of a twelve-inch bar magnet, as before described, once very quickly from the centre to the ends of I. and II.

$$C_p S_p = 4.09 \text{ inches; } C_p O_p = 1.41 \text{ inch; } C_u N_u = 2.79 \text{ inches;}$$

$$C_u N_u = 3.99 \text{ inches; } C_u O_u = 1.14 \text{ inch; } C_u S_u = 2.91 \text{ inches.}$$

Fig. 3. represents the positions of the same points, and of new poles S' , N'' . and magnetic centres O' , O'' , in consequence of a further disturbance of the magnetism, by drawing the end of the twelve-inch magnet twice from the centre to the ends of I. and II.

$C_I S_I = 4.26$ in.; $C_I O_I = 2.83$ in.; $C_I N_I = 0.41$ in.; $C_I O' = 2.57$ in.; $C_I S' = 4.32$ in.;
 $C_{II} N_{II} = 4.24$ in.; $C_{II} O_{II} = 2.34$ in.; $C_{II} S_{II} = 0.46$ in.; $C_{II} O'' = 3.31$ in.; $C_{II} N'' = 4.51$ in..

It is easily shown that such changes in the positions of these points will account for what are termed "the secondary deflections," arising from disturbance in the magnetism of the needle, according to the law that the horizontal needle will always assume the position of the projection on the horizontal plane of a needle freely suspended by its centre of gravity, that centre coinciding with the magnetic centre of the horizontal needle, and the iron attracting both poles of the inclined needle.

In the paper to which I have referred it is stated, that when the needle is "placed any where in the magnetic equator of the ball, whichever end of the needle has its magnetism deteriorated, that end will approach the ball."

According to my view of the subject, the centre of the spherical shell will, in this case, coincide with the equator of a freely suspended needle whose centre coincides with the centre of figure, and if the magnetism be equally distributed, of magnetism likewise of the horizontal needle; in which latter case the shell would produce no deviation in the freely suspended needle, and consequently none in the horizontal needle. But when either "branch is deteriorated," I have shown that the magnetic centre of the needle will recede from the centre of figure along the "undeteriorated branch," and the centre of the imaginary suspended needle coinciding with this centre, the centre of the shell will now be in the hemisphere on that side of the equator of the suspended needle in which is the "deteriorated branch." This branch then of the suspended needle, and consequently also, according to the law in question of the horizontal needle, will approach the shell.

The explanation is equally simple in other positions of the needle. The effects are stated to be these: "Generally, in other positions, one branch of the needle will be nearer to the centre of the ball than the other; then, if the near end have its magnetism deteriorated, the needle will approach its natural

meridian; but if the more distant end be deteriorated, the needle will be more deflected, or recede from the meridian."

It is evident that "if the near end have its magnetism deteriorated," the needle's magnetic centre will recede from the shell, and its deviation will nearly correspond to that of another horizontal needle having its magnetism symmetrically distributed in the two branches, and its centre coinciding with the magnetic centre of the former; the deviation will therefore in this case be diminished, or in other words, "the needle will approach its natural meridian." And "if the more distant end be deteriorated," it is clear that the magnetic centre will approach the shell, and consequently "the needle will be more deflected, or recede from the meridian."

This is, I consider, quite sufficient to show the general accordance of these experiments with the law to prove the fallacy of which they were brought forward; and I shall in the subsequent part of this paper, show that this law not only indicates clearly the nature of the deviations which in such cases take place, but that calculations founded upon it, give close approximations to their numerical values.

At the same time that I found the effects which have been described were invariably produced, on the positions of the poles and magnetic centre of the magnetized bars and needles by the disturbance of their magnetism, I likewise found that the intensities of both poles were greatly changed. With the three needles to which I have before referred, and which previous to the disturbance of their magnetism made ten vibrations in about thirty-four seconds, and after it, ten vibrations in about fifty-three seconds; the intensity of the pole in the branch on which the disturbing magnet had been applied, had decreased in about the ratio of five to two, and in the other of seven to four. But these effects I shall more particularly describe when I detail the observations by which I ascertained them.

With regard to the difference in the extent of the deviations of a needle six inches in length, and of one about a third of that length, when successively placed in the same position with respect to the shell, I found that at the distance 16.8 inches from the shell, this difference amounted in some instances to more than $2^{\circ} 30'$; the deviation being in the one case $13^{\circ} 38'$, and in the other $11^{\circ} 04'$: also that there was a sensible, though small difference in the deviations

of the shorter of these needles, and of one only an inch in length; the latter deviation being in the above situation only $10^{\circ} 51'$. I likewise found that even at the distance of twenty-four inches from the shell, the deviations of the longer needle were still sensibly different from those of either of the others. In general, when the needles were near to north or south from the shell's centre, the deviations of the longer needle exceeded those of the shorter; and the reverse took place when the needles were near to east or west from that centre. This is of course a very general description of the effects on the different needles, the precise nature of those effects will be best understood by consulting the tables containing the details of the observations. From this general description, however, we may draw a practical inference of some consequence; viz., that if the deviations that would take place in a needle from the influence of large and distant masses of iron, be counteracted by that of a small mass placed near to the needle, so that it retains its direction in the magnetic meridian in a certain position of the lines joining these masses and the centre of the needle with respect to that meridian; then the needle and these masses preserving their relative positions, if the whole revolve, the deviations caused by the large and distant mass will in some positions preponderate over those due to the smaller, and in some the contrary effect will be produced. It is therefore of importance that the counteracting mass of iron should be at such a distance from the needle that the difference of its effects upon a long needle and upon a very short one would be scarcely appreciable.

Having given a general view of some of the results which I obtained, I shall now deduce, according to the law which I have proposed, equations for the deviations of a horizontal needle due to the action of an iron sphere or shell, applicable to the different circumstances of the experiments which I shall afterwards detail.

Take the centre of the horizontal needle as the origin of three rectangular co-ordinates x, y, z to the centre of the sphere; z being in the direction of the terrestrial force, or the axis of a freely suspended needle concentric with the horizontal needle; y , in that of the magnetic meridian on the plane of the equator of this dipping needle; and x , in that of the intersection of this equator with the horizon: x being measured towards east, y towards north, and z downwards in the direction of the south pole of the dipping needle. Let the

distance of each pole of this dipping needle from its centre be e , or its length $2e$; m the terrestrial magnetic force acting upon each pole in the direction of the dip, and f the force of the sphere upon either pole at the unity of distance, supposing the whole mass collected in the centre. The square of the distance of the sphere's centre from the south pole of the dipping needle will be $x^2 + y^2 + (z - e)^2$; and from its north pole $x^2 + y^2 + (z + e)^2$.

So that the forces urging these poles towards the centre of the sphere will be

$$\frac{f}{x^2 + y^2 + (z - e)^2} \text{ and } \frac{f}{x^2 + y^2 + (z + e)^2}.$$

The deviation of the needle will take place in the plane passing through the centre of the sphere and the axis z ; and it is evident that it will be the same, if instead of the forces acting upon the north pole, we suppose forces equal to these, but in contrary directions, to act upon the south pole. If then we resolve these forces into others in the direction of the axis z , and perpendicular to it in the plane passing through z and the centre of the sphere, the south pole will be acted upon by a force in the direction z equal to

$$2m + \frac{f(z - e)}{\{x^2 + y^2 + (z - e)^2\}^{\frac{3}{2}}} - \frac{f(z + e)}{\{x^2 + y^2 + (z + e)^2\}^{\frac{3}{2}}};$$

and by a force in the direction perpendicular to it equal to

$$f(x^2 + y^2)^{\frac{1}{2}} \left\{ \frac{1}{\{x^2 + y^2 + (z - e)^2\}^{\frac{3}{2}}} - \frac{1}{\{x^2 + y^2 + (z + e)^2\}^{\frac{3}{2}}} \right\}.$$

Let R be the distance of the sphere's centre from that of the needle, and γ the angle which this distance makes with the axis z . If we substitute R^2 for $x^2 + y^2 + z^2$, $R \cos \gamma$ for z , and $R \sin \gamma$ for $(x^2 + y^2)^{\frac{1}{2}}$, in these expressions, and expand those which result, neglecting the terms containing powers of e higher than the third,

$$2m + \frac{fe}{R^3} \left[2 \cdot (\cos^2 \gamma - 1) + \frac{e^2}{R^2} \{ 3 - 5 \cos^2 \gamma (6 - 7 \cos^2 \gamma) \} \right]$$

will represent the force in the direction z ; and

$$\frac{fe}{R^4} \cdot \sin \gamma \cos \gamma \left\{ 2 \cdot 3 - \frac{e^2}{R^2} (3 \cdot 5 - 5 \cdot 7 \cos^2 \gamma) \right\},$$

the force perpendicular to z , in the plane passing through the axis z and the centre of the sphere.

If then ψ is the angle of deviation of the needle from the axis z , ψ will also be the angle which the resultant of these forces makes with that axis: we shall therefore have

$$\tan \psi = \frac{\sin \gamma \cos \gamma \left\{ 2.3 - \frac{e^2}{R^2} (3.5 - 5.7 \cos^2 \gamma) \right\}}{\frac{2mR^2}{fe} + 2(3 \cos^2 \gamma - 1) + \frac{e^2}{R^2} \{3 - 5 \cos^2 \gamma (6 - 7 \cos^2 \gamma)\}} \quad (1)$$

If e is extremely small, we may neglect the terms containing $\frac{e^2}{R^2}$, and the equation then becomes

$$\tan \psi = \frac{3 \sin \gamma \cos \gamma}{\frac{mR^2}{fe} + 3 \cos^2 \gamma - 1} \quad (2)$$

or

$$\tan \psi = \frac{3 \sin 2\gamma}{\frac{2mR^2}{fe} + 3 \cos 2\gamma + 1} \quad (3)$$

These equations give the position which an extremely short freely suspended needle would assume by the combined action of the iron sphere, and of terrestrial magnetism; and according to the law which I consider obtains, the projection of this inclined needle upon the horizontal plane will give the position which the horizontal needle will assume in consequence of the same action*.

The rectangular co-ordinates x, y, z determine the position of the iron sphere; but if ω be the angle which the projection of the radius vector R upon the plane xy makes with the axis y , its position will be determined by the polar co-ordinates R, γ, ω . Let the angle which the axis z makes with the vertical, or the complement of the dip, be represented by η , and the angle which the projection of the freely suspended needle upon the horizontal plane makes

* This principle, that the deviations of the horizontal needle may be referred to those of the inclined needle, I had pointed out a considerable time previous to my showing, in a paper in the Camb. Phil. Trans. for 1820, that it is consistent with experimental results. It was adopted in the first edition of Mr. BARLOW's "Essay on Magnetic Attractions," and likewise in the theoretical investigations in the second edition of that work. M. POISSON, in his "Mémoire sur la Théorie du Magnétisme," published in 1824, employs the same principle.

with the projection of the axis z upon the same plane or the magnetic meridian, and which, according to the principle adopted, is the deviation of the horizontal needle, be represented by ϕ : then the intersection of the axis z , of the vertical, and of the line of the inclined needle with the surface of a sphere described about the centre of the needle, will be the angular points of a spherical triangle, of which two sides are η , ψ , their included angle $\pi - \omega$, and ϕ the angle opposite to ψ . We shall therefore have

$$\sin \omega \cdot \cot \phi = \sin \eta \cot \psi + \cos \omega \cos \eta;$$

from which and the equation (3), we obtain

$$\cot \phi = \frac{\sin \eta \left\{ \frac{2mR^2}{fe} + 3 \cos 2\gamma + 1 \right\}}{3 \sin 2\gamma \sin \omega} + \cos \eta \cot \omega. \quad (4)$$

If we determine the position of the iron sphere by polar co-ordinates relative to the vertical and the plane of the horizon, calling the angle which the radius R makes with the vertical v , and the angle which its projection on the plane of the horizon makes with the magnetic meridian, or the azimuth of the iron sphere, θ ; then v , γ , η will be the sides of a spherical triangle of which the angles opposite to v and γ are $\pi - \omega$ and θ . We have therefore

$$\cos \gamma = \cos \theta \cdot \sin \eta \cdot \sin v + \cos \eta \cdot \cos v,$$

$$\sin \omega \cdot \sin \gamma = \sin \theta \cdot \sin v,$$

$$\cot \theta \sin \omega = \cot \gamma \sin \eta + \cos \omega \cos \eta.$$

From these, putting the equation (4) in the form

$$\cot \phi = \frac{\sin \eta \cdot \left(\frac{mR^2}{fe} - 1 \right)}{3 \sin \gamma \cos \gamma \sin \omega} + \frac{\cot \gamma \sin \eta + \cos \omega \cos \eta}{\sin \omega},$$

we obtain

$$\cot \phi = \frac{\frac{mR^2}{fe} - 1}{3 \sin \theta \sin v (\cos \theta \sin v + \cot \eta \cos v)} + \cot \theta. \quad (5)$$

If, as is most convenient for experiment, the position of the iron sphere be determined by its vertical distance below the horizontal plane on which the needle is, and its horizontal distance from the centre of the needle, let r be the projection of R on the horizontal plane, and h the distance of the sphere's centre below that plane: then since, $\sin v = \frac{r}{R}$, and $\cos v = \frac{h}{R}$,

$$\cot \phi = \frac{\left(\frac{m R^3}{f e} - 1\right) \cdot R^2}{3 r \sin \theta (r \cos \theta + h \cot \gamma)} + \cot \theta \quad (6)$$

This when $h = 0$, or the centre of the sphere is in the same horizontal plane as that of the needle, becomes

$$\cot \phi = \frac{2 \left(\frac{m R^3}{f e} - 1\right)}{3 \sin 2 \theta} + \cot \theta \quad (7)$$

from which the values of $\frac{m}{f e}$ will be determined in different magnetic latitudes from the observed values of ϕ , independent of the dip.

We may, in a similar manner, deduce an equation for the deviations of a needle in which the magnetism has been disturbed by applying to one of its poles the corresponding pole of a magnet; and thence determine the deviations of a needle having its magnetism symmetrically distributed in its two branches from the observed deviations of a needle in which such distribution has been disturbed. I have already stated that under these circumstances the intensities of both poles were changed, and it will be seen by the experiments detailed in a subsequent part of this paper, that their distances from the new magnetic centre were not the same, the more energetic pole being the nearer; and I will assume that the same is the case with the needle freely suspended from a point which, as before, is its centre of gravity and of magnetism. Let the quantities which before were indicated by m, f, e be indicated by m', f', e' for the north or upper pole of the suspended needle, and by m, f, e for its south or lower pole. We will also suppose that the south pole, or that which is usually marked, is that to which the corresponding pole of a magnet has been applied, or that "the deteriorated branch" is the northern; and that R_1 is the distance of the sphere's centre from the magnetic centre of this needle, γ_1 the angle which R_1 makes with the axis z passing through this centre, and ψ_1 the angle which the freely suspended needle makes with the same axis, or its deviation due to the action of the iron sphere. The equation corresponding to (2) will now become

$$\tan \psi_1 = \frac{\sin \gamma_1 \{R_1(f' e' - f_i e_i) + 3 \cos \gamma_1 (f' e'^2 + f_i e_i^2)\}}{(m' e' + m_i e_i) \cdot R_1^2 + \cos \gamma_1 \{R_1(f' e' - f_i e_i) + 3 \cos \gamma_1 (f' e'^2 + f_i e_i^2)\} - (f' e'^2 + f_i e_i^2)}$$

Now $\frac{m'}{f'} = \frac{m_i}{f_i} = \frac{m}{f}$; and it will be seen by the experiments that the values

of the products of the intensity of each pole by its distance from the magnetic centre are so nearly the same, that, as an approximation, we may assume them equal; and supposing the same distribution in the inclined needle, we have

$$\frac{e}{e_1} = \frac{f}{f_1} = \frac{m}{m_1}. \quad \text{Consequently, } \tan \psi_1 = \frac{3 \sin \gamma_1 \cos \gamma_1}{\frac{2e}{e_1 + e} \cdot \frac{m R_1^3}{f e} + 3 \cos^2 \gamma_1 - 1} \quad (8)$$

If then δ_1 is the azimuth of the iron sphere's centre from the magnetic centre of the deteriorated horizontal needle; ϕ_1 the deviation of this needle from the magnetic meridian; and r_1 the projection of R_1 upon the horizontal plane; we have, putting p for $\frac{2e}{e_1 + e}$,

$$\cot \phi_1 = \frac{\left(\frac{p m R_1^3}{f e} - 1\right) \cdot R_1^3}{3 r_1 \sin \delta_1 (r_1 \cos \delta_1 + h \cot \eta)} + \cot \delta_1 \quad (9)$$

Let k be the distance of the magnetic centre of the deteriorated needle from its centre of figure, which distance, as will be seen from the experiments, is to be measured towards the south when the northern branch of the needle is that "deteriorated;" then

$$r_1^2 = r^2 + k^2 + 2 r k \cos (\theta - \phi_1); \quad R_1^2 = R^2 + k^2 + 2 r k \cos (\theta - \phi_1);$$

$$r_1 \sin \delta_1 = r \sin \theta + k \sin \phi_1; \quad r_1 \cos \delta_1 = r \cos \theta + k \cos \phi_1.$$

Substituting these values in the equation (9), and limiting the approximation to the first power of $\frac{k}{r}$, we have,

$$\cot \phi_1 = \frac{p m R^3}{f e} \cdot \frac{R^3}{3 r Q} \cdot \left\{ 1 + \left(\frac{5 r^2}{R^2} \cdot \cos (\theta - \phi) - \frac{S}{Q} \right) \cdot \frac{k}{r} \right\} - \frac{R^3}{3 r Q} \left\{ 1 + \left(\frac{2 r^2}{R^2} \cdot \cos (\theta - \phi) - \frac{S}{Q} \right) \cdot \frac{k}{r} \right\} + \cot \theta + \frac{\sin (\theta - \phi)}{\sin^2 \theta} \cdot \frac{k}{r}$$

where $Q = \sin \theta \cdot (r \cos \theta + h \cot \eta)$, and $S = r \sin (\theta + \phi) + h \cot \eta \sin \phi$.

Putting for $\frac{p m R^3}{f e} \cdot \frac{R^3}{3 r Q}$ its value from the equation (6), $\cot \phi - \cot \theta + \frac{R^3}{3 r Q}$, we obtain

$$\cot \phi = \frac{\cot \phi + (p-1) \cdot \left\{ \cot \theta - \frac{R^3}{3 r Q} \right\} + \left[p \cdot \left\{ \cot \theta - \frac{R^3}{3 r Q} \right\} \cdot \left\{ \frac{5 r^2}{R^2} \cos (\theta - \phi) - \frac{S}{Q} \right\} + \frac{R^3}{3 r Q} \cdot \left\{ \frac{2 r^2}{R^2} \cos (\theta - \phi) - \frac{S}{Q} \right\} - \frac{\sin (\theta - \phi)}{\sin^2 \theta} \right] \cdot \frac{k}{r}}{p \cdot \left[1 + \left\{ \frac{5 r^2}{R^2} \cos (\theta - \phi) - \frac{S}{Q} \right\} \cdot \frac{k}{r} \right]}$$

or

$$\cot \phi = \frac{\cot \phi - \left(\cot \theta - \frac{R^3}{3 r Q} \right) + \left[\frac{R^3}{3 r Q} \cdot \left\{ \frac{5 r^2}{R^2} \cos (\theta - \phi) - \frac{S}{Q} \right\} - \frac{\sin (\theta - \phi)}{\sin^2 \theta} \right] \cdot \frac{k}{r}}{p \cdot \left[1 + \left\{ \frac{5 r^2}{R^2} \cos (\theta - \phi) - \frac{S}{Q} \right\} \cdot \frac{k}{r} \right]} + \cot \theta - \frac{R^3}{3 r Q} \quad (10),$$

the latter being rather more convenient for computation.

From this equation the values of ϕ , or the deviations of a needle having its

magnetism similarly distributed in its two branches, may be computed from the observed values of ϕ , or the observed deviations of a needle having its magnetism dissimilarly distributed.

The equation (10) admits of considerable simplification when the centre of the shell is in the same horizontal plane as that of the needle. In this case $h = 0$; r becomes R ; $Q = R \sin \theta \cos \theta$; $S = R \sin (\theta + \phi)$; and consequently,

$$\cot \phi = \frac{\cot \phi_i - \frac{3 \cos^2 \theta - 1}{3 \sin \theta \cos \theta} + \left[\frac{1}{3 \sin \theta \cos \theta} \left\{ 2 \cos (\theta - \phi_i) - \frac{\sin (\theta + \phi_i)}{\sin \theta \cdot \cos \theta} \right\} - \frac{\sin (\theta - \phi_i)}{\sin^2 \theta} \right] \cdot \frac{k}{R}}{p \cdot \left[1 + \left\{ 3 \cos (\theta - \phi_i) - \frac{\sin (\theta + \phi_i)}{\sin \theta \cos \theta} \right\} \cdot \frac{k}{R} \right]} + \frac{3 \cos^2 \theta - 1}{3 \sin \theta \cos \theta},$$

or since
$$2 \cos (\theta - \phi_i) - \frac{\sin (\theta + \phi_i)}{\sin \theta \cos \theta} = 2 \cot 2 \theta \cdot \sin (\theta - \phi_i),$$

$$\cot \phi = \frac{\cot \phi_i - \frac{3 \cos 2 \theta + 1}{3 \sin 2 \theta} - \frac{2 (\cos 2 \theta + 3)}{3 \sin^2 2 \theta} \cdot \sin (\theta - \phi_i) \cdot \frac{k}{R}}{p \cdot \left[1 + \left\{ 3 \cos (\theta - \phi_i) + 2 \cot 2 \theta \cdot \sin (\theta - \phi_i) \right\} \cdot \frac{k}{R} \right]} + \frac{3 \cos 2 \theta + 1}{3 \sin 2 \theta} \quad (11)$$

If ϕ and ϕ_i have both been determined from observation, the value of p may be determined from this equation,

$$p = \frac{\cot \phi_i - \frac{3 \cos 2 \theta + 1}{3 \sin 2 \theta} - \frac{2 (\cos 2 \theta + 3)}{3 \sin^2 2 \theta} \cdot \sin (\theta - \phi_i) \cdot \frac{k}{R}}{\left\{ \cot \phi - \frac{3 \cos 2 \theta + 1}{3 \sin 2 \theta} \right\} \cdot \left[1 + \left\{ 3 \cos (\theta - \phi_i) + 2 \cot 2 \theta \cdot \sin (\theta - \phi_i) \right\} \cdot \frac{k}{R} \right]} \quad (12)$$

The equations (10), (11), (12) have been obtained on the supposition that the disturbing magnet has been applied to the northern branch of the needle, or that the northern branch is "deteriorated," in which case the magnetic centre will be found between the centre of figure and the southern extremity; but if the disturbing magnet is applied to the southern branch, or it is this branch which is "deteriorated," the magnetic centre will be found between the centre of figure and the northern extremity; and therefore in applying these equations to the latter case, k must be considered negative.

Having deduced equations by which the deviations of a horizontal needle, due to the action of an iron sphere or shell, may be computed, both when the distribution of magnetism is similar, and when it is dissimilar in the two branches of the needle, I shall now proceed to the detail of experiments, and to their comparison with these theoretical results.

I have already stated that circumstances led me to suspect the accuracy of

the observations from which the conclusion had been drawn, that a mass of soft iron will cause the same deviation in a magnetized needle of six inches, and in one of an inch and a half in length; or in other words, that, within these limits, the deviations are independent of the needle's length; and the first observations which I made, not only confirmed my suspicions, but placed the matter beyond doubt. The apparatus which I made use of is nearly the same as that which I employed for the experiments by which I first showed, that the deviations of a horizontal needle due to the action of a mass of iron are in conformity with the law in question. In those experiments I employed a shell 12.78 inches in diameter: the diameter of that used in the experiments I am about to describe is 17.7 inches. I was indebted for the use of this shell to Sir William Congreve, who allowed me to select it from a few of that diameter in the Repository at Woolwich. This shell can be lowered or raised by means of pulleys, and passes through a circular hole in a horizontal table. On this table I had described two circles concentric with the shell, when its centre was in the plane of the table, one 9.5 inches radius, the other 28 inches; and these circles I had divided very carefully to thirds of a degree: so that, a very fine wire being stretched from a division in the one to the corresponding division in the other, the compass can be adjusted with considerable accuracy to any azimuth from the shell's centre, by means of indexes outside the box, corresponding to 0 and 180 on the divided ring. This ring is divided to 20'; and as I had made the needles to fit it very accurately, and had terminated them by very sharp points, I consider that I could read the deviations pretty accurately to 2' by means of a lens; and as in all cases, the readings at both ends of the needle were registered and a mean taken, the errors of centering were avoided, and the whole amount of error diminished. When the shell had been lowered so that its vertical distance above or below the plane of the table was that at which the observations were to be made, I very carefully adjusted it, by means of plummets suspended from very fine wires crossing over it, so that the vertical through its centre passed through the centre of the concentric graduated circles on the horizontal table.

To determine the influence which the length of a needle has on its deviations, I made use of three different needles, which I had marked A, D, P. A is 6.01 inches in length, bounded by circular arcs, its greatest breadth being 0.52 inch; D is 1.87 inch in length, its breadth 0.22 inch; P is 1.03 inch in

length, and 0.19 inch in breadth; the forms of D and P being nearly similar to that of A. The weight of A is 77.5 grains; of D 7.0 grains; and of P 7.75 grains. To the needles D and P were attached slips of mica, of the same length as A, having fine lines drawn on them, and with which the axes of the needles were made to coincide; so that I could observe the corresponding deviations of the three needles by placing them successively on the pivot of the same compass-box, which therefore remained in the same position for each, after it had been so adjusted that the centre of the shell had the required azimuth with respect to that of the needle. With these needles, observations were made in three different positions of the shell; viz. when its centre was ten inches above the horizontal plane passing through the needle's centre; when its centre coincided with; and when ten inches below that plane. When the shell was above or below that plane, the horizontal distance of the needle's centre from the vertical passing through that of the shell was 13.5 inches; so that placing the needle in different azimuths on the horizontal table, had the same effect as making the shell describe a parallel to the horizon in a sphere 16.8 inches radius described about the needle. When the centre of the shell was in the horizontal plane passing through the needle's centre, the distance between these centres was 16.8 inches. In all these cases the deviations of the three needles were observed at every 20° of azimuth round the circle. These distances were adopted as they are those selected in the experiments described in the paper, "On the secondary deflections produced in a magnetized needle by an iron shell, &c." in the Transactions of last year; and I had consequently computed the values of the angles γ , ω , in the equations (3), (4), according to such adjustments. Whatever errors might be made in the adjustment of the compass,—and I have no hesitation in saying that these were always of small amount,—the relative deviations of the three needles for each azimuth were independent of these errors, since the compass-box remained in the same position for the corresponding observations with each needle.

The following Tables contain the deviations of the north end of the needle, deduced from the readings at both ends, in three different sets of experiments; in each of which all the adjustments of the height of the shell, its centering, the azimuth and distance of the compass, were made afresh. The mean of the deviations corresponding to each azimuth of the shell to the west is taken, and likewise of those corresponding to each azimuth to the east;

and again the mean of these mean deviations in opposite directions for each azimuth. In these, + indicates that the deviation of the north end of the needle is of the same name as the azimuth of the shell, or that it is towards the shell; and -, that it is of a contrary name, or from the shell.

The centre of the Shell 10 inches below the horizontal plane passing through the Needle's centre, and its distance from that centre 16.8 inches.												
Needle of which the deviation was observed.	Azimuth of the Shell's centre from North.	The Azimuth of the Shell's centre from that of the Needle being										Means of the deviations with the Shell West and with the Shell East.
		West.					East.					
		Deviations of the North end.			Means.		Deviations of the North end.			Means.		
A. Length = 6.01 inches.	20	10 46W	10 46W	10 48W	10 44 46W	10 40E	10 32E	10 33E	10 35 00E	+10 39 50		
	40	20 24	20 34	20 30	20 29 20	20 24	20 14	20 17	20 18 20	20 23 50		
	60	28 02	28 06	28 10	28 06 00	28 05	27 55	27 55	27 58 20	28 02 10		
	80	32 23	32 27	32 29	32 26 20	32 47	32 41	32 46	32 44 40	32 35 30		
	100	32 00	32 08	32 08	32 05 20	32 40	32 35	32 27	32 34 00	32 19 40		
	120	27 50	28 09	27 58	27 59 00	28 18	28 18	28 08	28 14 40	28 06 50		
	140	22 02	22 11	21 56	22 03 00	22 19	22 21	22 11	22 17 00	22 10 00		
160	13 34	13 46	13 21	13 33 40	13 32	13 31	13 18	13 27 00	13 30 20			
D. Length = 1.57 inch.	20	9 50	10 00	9 57	9 55 40	9 54	9 47	9 46	9 49 00	9 52 20		
	40	19 01	19 10	19 02	19 04 20	19 05	18 56	18 52	18 57 40	19 01 00		
	60	26 32	26 36	26 34	26 34 00	26 31	26 27	26 22	26 26 40	26 30 20		
	80	31 31	31 36	31 35	31 34 00	31 49	31 46	31 45	31 46 40	31 40 20		
	100	32 51	32 57	32 52	32 53 20	33 16	33 12	33 06	33 11 20	33 02 20		
	120	29 29	29 44	29 31	29 34 40	29 42	29 44	29 35	29 40 20	29 37 30		
	140	21 36	21 49	21 36	21 40 20	21 51	21 51	21 53	21 51 40	21 46 00		
160	10 58	11 10	10 59	11 02 20	11 07	10 58	10 56	11 00 20	11 01 20			
F. Length = 1.03 inch.	20	9 51	9 56	9 54	9 53 40	9 50	9 42	9 48	9 46 40	9 50 10		
	40	18 57	19 07	18 57	19 00 20	19 02	18 49	18 53	18 54 40	18 57 30		
	60	26 28	26 34	26 28	26 30 00	26 30	26 20	26 24	26 24 40	26 27 20		
	80	31 31	31 40	31 30	31 33 40	31 53	31 41	31 48	31 47 20	31 40 30		
	100	32 57	33 03	32 56	32 58 40	33 23	33 21	33 14	33 19 20	33 09 00		
	120	29 36	29 54	29 33	29 41 00	29 46	29 49	29 46	29 47 00	29 44 00		
	140	21 46	21 49	21 32	21 42 20	21 46	21 49	21 56	21 50 20	21 46 20		
160	10 55	11 00	10 48	10 54 20	10 46	10 42	10 40	10 42 40	10 48 30			

The centre of the Shell in the horizontal plane passing through the Needle's centre, and its distance from that centre 16.8 inches.											
Needle of which the deviation was observed.	Azimuth of the Shell's centre from North.	The Azimuth of the Shell's centre from that of the Needle being								Means of the deviations with the Shell West and with the Shell East.	
		West.				East.					
		Deviations of the North end.		Means.		Deviations of the North end.		Means.			
A. Length = 6.01 inches.	20	7 14W	7 16W	7 20W	7 16 40W	7 07E	7 16E	7 13E	7 12 00E	+	7 14 20
	40	11 45	11 46	11 52	11 47 40	11 41	11 34	11 35	11 36 40		11 42 10
	60	10 20	10 33	10 24	10 25 40	10 06	10 03	10 06	10 05 00		10 15 20
	80	3 46	3 55	3 53	3 51 20	3 30	3 30	3 22	3 27 20		3 39 20
	100	4 30E	4 21E	4 24E	4 25 00E	4 42W	4 34W	4 39W	4 38 20W	-	4 31 40
	120	11 05	11 04	10 58	11 02 20	11 22	11 16	11 28	11 22 00		11 12 10
	140	12 07	12 07	12 04	12 06 00	12 13	12 17	12 10	12 13 20		12 09 40
160	7 20	7 19	7 20	7 19 40	7 38	7 45	7 38	7 40 20		7 30 00	
D. Length = 1.97 inch.	20	6 13W	6 17W	6 22W	6 17 20W	6 07E	6 11E	6 08E	6 08 40E	+	6 13 00
	40	10 26	10 34	10 39	10 33 00	10 29	10 18	10 18	10 21 40		10 27 20
	60	10 17	10 26	10 24	10 22 20	9 59	9 58	9 58	9 58 20		10 10 20
	80	4 24	4 40	4 38	4 34 00	4 01	4 09	4 02	4 04 00		4 19 00
	100	4 40E	4 27E	4 19E	4 28 40E	4 54W	4 44W	4 53W	4 50 20W	-	4 39 30
	120	10 27	10 22	10 19	10 22 40	10 41	10 40	10 43	10 41 20		10 32 00
	140	10 25	10 24	10 23	10 24 00	10 34	10 38	10 38	10 36 40		10 30 20
160	6 06	6 04	6 05	6 05 00	6 31	6 39	6 36	6 35 20		6 20 10	
F. Length = 1.03 inch.	20	6 12W	6 12W	6 16W	6 13 20W	6 01E	6 12E	6 13E	6 08 40E	+	6 11 00
	40	10 24	10 28	10 34	10 28 40	10 20	10 18	10 22	10 20 00		10 24 20
	60	10 10	10 25	10 17	10 17 20	9 55	10 04	10 05	10 01 20		10 09 20
	80	4 29	4 37	4 36	4 34 00	4 04	4 14	4 05	4 07 40		4 20 50
	100	4 37E	4 28E	4 26E	4 30 20E	5 00W	4 47W	4 50W	4 52 20W	-	4 41 20
	120	10 21	10 21	10 18	10 20 00	10 41	10 33	10 37	10 37 00		10 28 30
	140	10 16	10 21	10 25	10 20 40	10 30	10 29	10 26	10 28 20		10 24 30
160	5 59	6 03	6 06	6 02 40	6 21	6 29	6 25	6 25 00		6 13 50	

The centre of the Shell 10 inches above the horizontal plane passing through the Needle's centre, and its distance from that centre 16.8 inches.												
Needle of which the deviation was observed.	Azimuth of the Shell's centre from North.	The Azimuth of the Shell's centre from that of the Needle being										Means of the deviations with the Needle West and with the Shell East.
		West.					East.					
		Deviations of the North end.		Means.			Deviations of the North end.		Means.			
A. Length = 6.01 inches.	20	13 00E	12 51E	13 04E	12 58 20E	12 32W	12 13W	12 32W	12 25 40W		12 42 00	
	40	21 41	21 26	21 34	21 33 40	21 07	20 40	21 10	20 59 00		21 16 20	
	60	28 09	28 03	27 57	28 03 00	27 32	27 04	27 35	27 23 40		27 43 20	
	80	32 38	32 34	32 15	32 29 00	31 51	31 27	31 50	31 42 40		32 05 50	
	100	32 54	32 54	32 43	32 50 20	32 12	31 56	32 13	32 07 00		32 28 40	
	120	28 04	28 16	28 06	28 08 40	27 54	27 50	27 56	27 53 20		28 01 00	
	140	20 06	20 04	20 02	20 04 00	20 04	20 07	20 09	20 06 40		20 05 20	
160	10 16	10 08	10 13	10 12 20	10 43	10 44	10 43	10 43 20		10 27 50		
D. Length = 1.87 inch.	20	11 00	10 52	10 59	10 57 00	10 39	10 20	10 25	10 28 00		10 42 30	
	40	21 31	21 19	21 20	21 23 20	21 04	20 34	20 55	20 51 00		21 07 10	
	60	29 49	29 46	29 21	29 38 40	29 13	28 44	29 19	29 05 20		29 22 00	
	80	33 16	33 18	32 54	33 09 20	32 38	32 07	32 43	32 29 20		32 49 20	
	100	31 50	31 50	31 40	31 46 40	31 27	31 04	31 25	31 18 40		31 32 40	
	120	26 30	26 35	26 28	26 31 00	26 20	26 20	26 32	26 24 00		26 27 30	
	140	18 39	18 38	18 35	18 37 20	18 38	18 47	18 50	18 45 00		18 41 10	
160	9 36	9 21	9 19	9 25 20	9 50	10 00	9 59	9 56 20		9 40 50		
F. Length = 1.03 inch.	20	11 05	10 59	11 02	11 02 00	10 24	10 11	10 12	10 15 40		10 38 50	
	40	21 31	21 26	21 20	21 25 40	21 01	20 35	20 53	20 49 40		21 07 40	
	60	29 41	29 50	29 31	29 40 40	29 17	28 36	29 10	29 01 00		29 20 50	
	80	33 21	33 15	33 03	33 13 00	32 31	32 08	32 33	32 24 00		32 48 30	
	100	31 40	31 39	31 36	31 38 20	31 15	31 01	31 18	31 11 20		31 24 50	
	120	26 23	26 26	26 23	26 24 00	26 09	26 13	26 14	26 12 00		26 18 00	
	140	18 31	18 30	18 34	18 31 40	18 38	18 43	18 41	18 40 40		18 36 10	
160	9 24	9 19	9 22	9 21 40	9 56	9 55	9 54	9 55 00		9 38 20		

I did not consider it necessary to make corresponding observations to these at an increased distance between the centres of the shell and needle at every 20° of azimuth; I however made the following observations in those positions where the differences in the deviations of the needles are the greatest,

when that distance was increased to twenty-four inches. The results show that at this distance the differences, though small, are still appreciable.

Needle of which the deviation was observed.	Azimuth of the Shell's centre from North.	The centre of the Shell 24 inches from that of the Needle, and											
		14.29 inches below the horizontal plane passing through the centre of the Needle.						14.29 inches above the horizontal plane passing through the centre of the Needle.			In the horizontal plane passing through the centre of the Needle.		
		Azimuth		Mean Deviation.	Azimuth		Mean Deviation.	Azimuth of Shell's centre from North.	Azimuth		Mean Deviation.		
		West.	East.		West.	East.			West.	East.			
Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.	Deviation.		
A.	20 60 120	10 45'W 8 10 2 55	10 50'E 8 13 2 42	+ 10 47½ 8 11½ 2 48½	2 47E 8 22 11 07	2 39'W 8 04 11 02	- 2 43 8 13 11 04½	40 140	3 46'W 3 57E	3 55E 4 00W	+ 3 51½ - 3 58½		
D.	20 60 120	10 29 8 10 2 40	10 52 8 18 2 25	10 40½ 8 14 2 32½	2 34 8 29 10 59	2 22 7 57 10 46	2 28 8 13 10 52½	40 140	3 33W 3 44E	3 57E 3 40W	+ 3 45 - 3 42		
P.	20 60 120	10 10 7 52 2 38	11 09 8 35 2 27	10 39½ 8 13½ 2 32½	2 38 8 49 11 18	2 15 7 41 10 27	2 26½ 8 15 10 52½	40 140	3 09W 3 04E	4 18E 3 21W	+ 3 43½ - 3 42½		

The differences in the deviations of the three needles in the foregoing tables may perhaps partly be attributed to the difference of their masses; and this probably produced some slight effect, especially at the nearer distance. This effect, arising from the shell becoming magnetic by the influence of the needle itself, would be in all cases to make the nearer end of the needle deviate towards the shell; and consequently in the position where the difference in the deviations of the long and short needles was observed to be greatest, it would be contrary to that observed, and must therefore have diminished the difference according to the extent to which the cause operated. These observations therefore show beyond doubt, that the length of a needle has a very sensible influence on the extent of its deviations. Although I did not consider that the difference in the masses of the needles had much effect in diminishing the difference of their deviations in some cases, or in increasing it in others, yet in order to decide what part of the difference was due to mass, and what to length, it was my intention, could I have found leisure, to have made observations corresponding to the foregoing with the needles A and P, and

two others six inches in length, one extremely light and the other much heavier than A.

In order to compare the theoretical results which we have obtained, that is the equations (3) and (6), with the observed deviations of the three needles, I take a mean of the deviations with the shell below the horizontal plane of the needle, and with it above, when the azimuth of its centre from south in the second case was the same as its azimuth from north in the first, as the values of ϕ corresponding to the different azimuths with the shell's centre below the horizontal plane of the needle: and likewise with the shell's centre in the horizontal plane of the needle, I take a mean of the deviations in the corresponding azimuths from north and south as the values of ϕ in this case. From the values of h, r, η^*, θ are derived those of γ and ω , and thence, from the values of ϕ , may be computed the corresponding values of ψ or the deviations of a freely suspended needle; and the value of the constant $\frac{2mR^3}{fe}$ will be determined from the equation,

$$\tan \psi = \frac{3 \sin 2\gamma}{\frac{2mR^3}{fe} + 3 \cos 2\gamma + 1};$$

or we have

$$\frac{2mR^3}{fe} = \frac{6r}{R^3} (r \cos \theta + h \cot \eta) \cdot \frac{\sin(\theta - \phi)}{\sin \phi} + 2,$$

from the equation (6), without the previous computation of γ, ω, ψ . The values of γ and ω corresponding to the different adjustments of the shell, the values of ψ and of the constant $\frac{2mR^3}{fe}$, computed from the means of the observed values of ϕ , with the different needles, are contained in the following Table.

The vertical distance of the centre of the Shell from that of the needle or $h = 10$ inches, $r = 13.5$ inches, and $R = 16.8$ inches.															Mean values of $\frac{2mR^3}{fe}$											
Deviations with	$\theta =$	20	00	00	40	00	00	60	00	00	80	00	00	100	00	00	120	00	00	140	00	00	160	00	00	
	$\gamma =$	35	09	21	39	39	30	45	50	00	52	37	23	54	59	13	45	65	02	40	69	35	11	72	28	41
	$\omega =$	28	30	34	54	01	48	75	57	51	95	13	54	112	55	33	129	51	52	146	33	20	163	14	58	
	$\phi =$	10	33	50	20	14	35	28	01	35	32	32	05	32	12	45	27	55	05	21	43	10	13	06	10	
Values of ψ with	$D =$	9	46	35	18	51	05	26	28	55	31	36	30	32	55	50	29	29	45	21	26	35	10	51	55	
	$P =$	9	44	15	18	46	50	26	22	40	31	32	40	32	58	45	23	32	25	21	27	00	10	43	40	
	$D =$	11	09	37	11	45	42	12	06	19	11	43	59	10	35	54	9	28	04	8	57	59	10	95	19	
	$P =$	9	58	36	10	39	07	11	15	00	11	21	01	10	49	44	9	54	08	8	53	14	8	06	22	
Values of $\frac{2mR^3}{fe}$ with	$P =$	9	55	13	10	35	49	11	11	37	11	19	27	10	50	40	9	54	52	8	53	21	8	02	27	
	$A =$	12.307												14.555		14.694				13.701		13.225		13.356		
	$D =$	14.047											14.208		14.218		14.079			13.813		13.551		14.023		
	$P =$	14.140											14.242		14.198		14.063			13.810		13.650		14.068		

* I take $\eta = 20^\circ$, the most recent observation giving the dip at this place $70^\circ 00'$.

The centre of the Shell in the same horizontal plane as that of the Needle or $h = 0$, and $R = 16.68$ inches.															
	δ	=	20	00	00	40	00	00	60	00	00	80	00	00	
	γ	=	71	15	10	74	48	40	80	09	12	86	35	42	
	ω	=	21	10	22	41	45	48	61	31	08	80	35	31	Mean values
Deviations with	A	=	7	22	10	11	55	55	10	43	45	4	05	30	$\frac{2mR^3}{fe}$
	D	=	6	16	35	10	28	50	10	21	10	4	29	15	
	P	=	6	12	25	10	24	25	10	18	55	4	31	05	
Values of ψ with	A	=	10	07	10	7	56	38	4	39	57	1	26	12	$\frac{2mR^3}{fe}$
	D	=	8	05	06	6	43	38	4	28	55	1	34	40	
	P	=	7	57	51	6	40	04	4	27	49	1	35	19	
Values of $\frac{2mR^3}{fe}$ with	A	=	11.612			12.460			14.212			16.163		13.612	$\frac{2mR^3}{fe}$
	D	=	14.235			14.450			14.722			14.893		14.575	
	P	=	14.433			14.565			14.775			14.804		14.644	

From these values of $\frac{2mR^3}{fe}$, with the different needles, it is evident that if the mean value with the needle A were substituted in the equation (6), the computed values of ϕ would not differ greatly from the observed values; but that with the needles D and P, the approximation would be so close, that the differences would clearly be within the limits of errors of observation. There can therefore be no doubt that the law which I proposed will give an extremely close approximation to experimental results, when the length of the needle does not bear too sensible a proportion to its distance from the centre of the attracting mass of iron; and that even if this ratio is not less than 1 to 3, the theoretical and experimental results will not differ greatly from each other.

I will now, for the purpose of comparison, deduce from these experiments the values of the constant depending upon the law, that "the tangent of the deviation is proportional to the rectangle of the cosine of the longitude, and the sine of the double latitude." If λ is the latitude of the needle's centre from a great circle of the shell perpendicular to the dip; and l its longitude measured from the intersection of this circle with the horizontal plane through the shell's centre; δ the dip of the needle; and M a constant,—then the law is

$$\tan \phi = \frac{\sin 2\lambda \cos l}{M \cos \delta} :$$

in which equation λ , l , δ are the complements of γ , ω , α in the foregoing equations.

The values of M determined by means of this equation from the mean of the

observed values of ϕ in the different azimuths, with the three needles, are contained in the Tables below.

The vertical distance of the centre of the Shell from that of the Needle or $h = 10$ inches, $r = 13.5$ inches, and $R = 16.8$ inches.									
Values of M with	$\theta =$	20°	40°	60°	80°	100°	120°	140°	160°
	A=	21.137	18.916	15.980	13.211	11.273	9.721	7.934	6.236
	D=	22.877	20.431	17.074	13.694	10.966	9.105	8.047	7.566
	P=	22.970	20.514	17.152	13.728	10.945	9.089	8.044	7.662

The centre of the Shell in the same horizontal plane as that of the Needle or $h = 0$, and $R = 16.8$ inches.					
Values of M with	$\theta =$	20°	40°	60°	80°
	A=	14.910	13.981	13.712	14.344
	D=	17.533	15.971	14.222	13.074
	P=	17.731	16.086	14.275	12.986

Here it appears that the values of M derived from these experiments, instead of being constant or nearly so, as they ought to be, vary from 6 to 21 with the long needle, and with the short ones from 8 to 23. The results are most consistent when the centres of the shell and needle are in the same horizontal plane; and within certain limits of the values of l , l being likewise constant throughout a set of observations, results that should not differ so very widely as the foregoing might be obtained: it is somewhat singular that the truth of the law should have been considered to be established experimentally from observations made in precisely these relative positions of the shell and needle*. When the centre of the shell and needle are in the same horizontal plane, the values of M deduced from the observations with the long needle A, agree much more nearly with each other than those deduced from the observations with either of the shorter needles D or P; and when they are not in the same plane, the disagreement in the values of M is nearly the same with each of the needles: we may therefore infer, that the proximity of the needle to the shell is not the cause of this disagreement, and that the same would be found to be the case if the values of M were deduced from deviations of the needle observed at greater distances from the shell's centre.

* Mr. BARLOW's Magnetic Attractions, 2nd ed. pp. 34, 37, 38.

We must therefore conclude, that although the law that "the tangent of the deviation is proportional to the rectangle of the cosine of the longitude and the sine of the double latitude" has been derived from experiment, and considered as giving close approximations to experimental results*; yet as it is quite clear that no law giving such inconsistent results as appear here† can be considered as even approximately true, it must be wholly rejected.

On the principle which I have assumed, I have already given the general explanation of the phenomena observed with needles having their magnetism unequally distributed; I shall now describe the experiments which I made with such needles, with the view of comparing them with the equations that I should derive from this principle, according to the distribution of magnetism which I found to take place under different circumstances of disturbance, which equations, (10) and (11), have been already given. In the beginning of this paper I have described the needles made use of in these experiments, and the manner of magnetizing them. These needles are distinguished by the letters A, B, C; A being that made use of in the experiments already described, and B and C of precisely the same form and weight as A.

In order to determine nearly the situations of the magnetic centre, and of the points towards which the forces are directed near the ends, in these needles, which points may be considered as their poles nearly, I successively placed them on a wooden rectangular scale, so that their axes coincided with a line drawn through the middle parallel to its sides. This scale, which is graduated

* Mr. BARLOW's *Magnetic Attractions*, 2nd ed. pp. 38, 39.

† If we take a mean of the deviations with the undeteriorated needle in the observations given in the *Transactions* of last year, p. 285, in the corresponding azimuths with the shell above and below the table, the values of M will be these:

$\theta = 20^\circ$	40°	60°	80°	100°	120°	140°	160°
M =	17.65	14.51	12.38	10.81	10.22	8.51	6.61

The observation corresponding to $\theta = 20^\circ$, which would have given by much the greatest value of M, does not appear to have been made. These values are not quite so discordant as those resulting from my observations, but are equally conclusive against the law from which they are derived. The means of the deviations when the shell was in the same horizontal plane as the needle, give the following values of M.

$\theta = 20^\circ$	40°	60°	80°
M = 14.40	13.33	12.91	12.83,

which are rather more discordant than the corresponding values from my observations.

across to tenths of an inch, and can be made to revolve about a vertical axis, being placed horizontally so that the axis of the needle was at right angles to the meridian, a rectangular stand, carrying the small needle P level with the other, was passed along and in contact with the northern side of the scale, until the axis of P pointed accurately in the meridian. This occurred in two positions; in the one, the force of the needle under trial concurring with the terrestrial directive force; and in the other, being in direct opposition to it. The distances, from the centre of the needle, of the points to which the centre of P was in these cases opposite were noted; and the same was done when P was passed to the south of the rectangular scale: the mean of the respective distances gave the distances, from the centre, of the points towards which the forces are directed, or nearly those of the poles. The position of the magnetic centre was determined similarly, by placing the rectangular scale, carrying the needle under trial, so that the axis of this needle was in the magnetic meridian with the marked end towards north, and passing P along the eastern side until its axis was in the meridian with its marked end south, and then along the western side. In all cases the distance between the axis of the needle and the centre of P was 1.5 inch.

The positions of these points in each needle, so ascertained after it had been carefully magnetized by double touch, as I have before described, and the times of the needle's vibration, were as follow.

Needle.	Distances from the Needle's centre.			Time of making 10 vibrations.
	Of the south or marked pole.	Of the magnetic centre or zero.	Of the north or unmarked pole.	
A.	inches. 2.43	inches. 0.00	inches. 2.42	sec. 34.1
B.	2.39	0.04 U.	2.42	32.75
C.	2.35	0.07 U.	2.47	32.7

After making these observations, the magnetism of the needle A was undisturbed; that of B was disturbed by drawing the unmarked end of a 12-inch bar magnet from the centre to the unmarked end of the needle twice, a thin board being interposed; that is, the needle B had its "southern branch deteriorated;" and the needle C had its "northern branch deteriorated," by similarly drawing the marked end of the magnet twice from the centre to the marked

end. The situations of the poles and magnetic centre were ascertained, as before, immediately after making the first set of observations, shortly to be described, on the deviations of these needles, when the shell was above the table on which they were placed; likewise previously to commencing the second set, when the shell was below the table; and also after the observations with the shell's centre in the plane of the table were concluded. The time in which each needle made ten vibrations was likewise observed. The following are the results.

Needle.	Distances from the Needle's centre.			Time of making 10 vibrations.	Date.	Hour.
	Of the south or marked pole.	Of the magnetic centre or zero.	Of the north or unmarked pole.			
C.	inches.	inches.	inches.	sec.		
	1.41	1.06 U.	2.80	51.8	28 Nov.	6 P.M.
	1.43	1.06	2.80	50.1	3 Dec.	1 P.M.
	1.49	1.04	2.80	50.65	5 Dec.	3 P.M.
	Means 1.44	1.05	2.80	50.85		
A.	2.40	0.05 U.	2.44	35.1	28 Nov.	6 P.M.
	2.41	0.03	2.43	34.35	3 Dec.	1 P.M.
	2.42	0.025	2.43	35.3	5 Dec.	8 P.M.
	Means 2.41	0.035	2.433	34.92		
B.	2.74	0.71 M.	2.23	52.85	28 Nov.	6 P.M.
	2.73	0.70	2.26	51.4	3 Dec.	1 P.M.
	2.72	0.62	2.33	51.8	5 Dec.	11 A.M.
	Means 2.73	0.677	2.273	52.02		

In these the letter M placed after the distance of zero from the needle's centre, indicates that the distance was measured towards the marked end of the needle; and the letter U, that it was measured towards the unmarked end. The distances of the poles and magnetic centres, as determined on different days, show that only a small change took place in the situations of these points in the needles during the time of making the observations on their deviations*.

To determine the intensity of the free magnetism on each side of the centres

* With the view of ascertaining what degree of permanency this disturbed state of the magnetism in a needle may have, I made observations similar to these, at considerable intervals of time, with the two 9-inch bar magnets which I have already mentioned. The bars had been carefully and strongly magnetized by double touch on the 5th of November, when the positions of the poles and magnetic centres appeared to be as in the first observations to each magnet, on that day, in the following Table. The magnetism of the bars was then disturbed by passing the unmarked end of a 12-inch magnet from the centre of I, to its unmarked end, and the marked end of the same magnet from the centre to the

of force, and likewise the position of the points in these needles where the intensities of contrary names were equal, I adopted the method of COULOMB. For this purpose a small needle 0.72 inch in length, 0.15 inch in breadth, of a form somewhat similar to A, B, or C, and weighing 1.25 grain, which for distinction I name I, was suspended by a single fibre of the silk-worm within a glass cylinder attached to a high rectangular support of wood, in which is

marked end of II: after this, the second observations on the 5th of November were made. In the interval between these observations and the subsequent ones, care was taken that these bars were quite out of the reach of accidental disturbance in their magnetism.

Magnet.	Distances from the Bar's centre.			Date of observation.
	Of the south or marked pole.	Of the magnetic centre or zero.	Of the north or unmarked pole.	
I. { Magnetism undisturbed Magnetism disturbed at unmarked end	inches. 3.75	inches. 0.48 U.	inches. 3.80	5 Nov. 1827
	4.06	1.49 M.	2.73	5 Nov.
	4.10	1.31	2.84	12 Dec.
	4.12	1.32	2.86	23 April, 1828
	4.12	1.32	2.86	14 May
II. { Magnetism undisturbed Magnetism disturbed at marked end	3.73	0.01	3.76	5 Nov. 1827
	2.64	1.52	4.05	5 Nov.
	2.67	1.47	4.07	12 Dec.
	2.75	1.45	4.05	23 April, 1828
	2.82	1.37	4.05	14 May

It appears that some, though an inconsiderable change took place in the distribution of the magnetism of the bar I, from the 5th of November to the 12th of December, but that it has remained almost precisely in the same state from the 12th of December to the 14th of May; and it will probably continue in this state so long as it meets with no extraneous disturbance. In the latter interval the observations indicate some change in the magnetism at the marked end of II; but even here it is not to a great extent. In neither of these bars are there any indications of other poles than those whose situations I observed. I am not aware of any experiments having been made to ascertain whether in magnetized steel bars the action of the magnetic particles upon each other has a tendency to restore a symmetrical arrangement of their magnetism when such arrangement has been disturbed; but the results I have obtained show that if such tendency exist for a short time after this disturbance, it soon, at least in some cases, either ceases to exist, or is so feeble as not to overcome, or to overcome very slowly, the coercive force of the steel. In the bars which I employed, this coercive force could not be great, if it depend upon hardness, since they are softer than requisite for working with a file. We ought, however, to be in possession of more extended observations before we draw any general conclusions from such facts as I have stated.

sity of the magnetism in its different points, became known. The needle I was rendered extremely hard, and was strongly magnetized, in order that its magnetism should not be disturbed by that of the large needle. The distance between the centre of I and the axis of the large needle was in all cases 1.63 inch, so that in some positions the vibrations of I, with its own inertia alone to overcome, would have been too rapid for observation; and to obviate this, it had attached to it, in all the observations, a thin disc of mica. The needle I was vibrated to the north of the large needle, whose marked or south pole was always downwards. The time of vibration of I, corresponding to each point of the large needle was determined by two trials, and as these never differed by more than a fifth of a second, the time may be considered to have been determined at least within this limit. The following Table shows the times of vibration of I, when not affected by the needles A, B, C, and when points at different distances from the centres of these needles were horizontally opposite to the centre of I; also the intensity of the magnetism in these points, deduced from those times of vibration; + indicating south polarity, or that predominating in the marked end of the needle, and - indicating north polarity.

From the Needle's centre of figure towards its unmarked end.													Vibrating by the terrestrial force alone.
0.80 in.	0.60 in.	0.00 in.	1.035 in.	1.10 in.	1.12 in.		2.00 in.	2.30 in.	2.40 in.	2.60 in.	2.80 in.	2.99 in.	
57.7°	58.75°		107.7°	120.4°	126.0°		73.1°	65.9°	61.8°	59.7°	59.8°	61.6°	121.3°
+4.4560	+4.3981		+1.2790	+1.0234	+0.9345		-2.7763	-3.4160	-3.8843	-4.1634	-4.1485	-3.9066	1.0000
+3.4560	+3.3981		+0.2790	+0.0234	-0.0655		-3.7763	-4.4160	-4.8843	-5.1634	-5.1485	-4.9066	
		0.00 in.	0.025 in.	0.05 in.	0.075 in.	1.80 in.	2.00 in.	2.30 in.	2.43 in.	2.60 in.	2.80 in.	2.99 in.	
		108.6°	115.6°	120.1°	129.2°	47.3°	46.3°	45.9°	45.6°	45.8°	48.1°	50.3°	121.4°
		+1.3496	+1.1029	+1.0218	+0.8829	-6.5874	-6.9048	-6.9954	-7.0877	-7.0960	-6.3701	-5.8483	1.0000
		+0.3496	+0.1029	+0.0218	-0.1171	-7.5874	-7.9048	-7.9954	-8.0877	-8.0960	-7.3701	-6.8483	
0.615 in.	0.60 in.					2.00 in.	2.10 in.	2.30 in.	2.33 in.	2.40 in.	2.60 in.	2.80 in.	2.99 in.
122.8°	126.8°					84.3°	83.55°	83.25°	83.50°	83.85°	85.5°	89.0°	94.1°
+0.9854	+0.9943					-2.0960	-2.1287	-2.1441	-2.1313	-2.1135	-2.0327	-1.8760	-1.6781
-0.0146	-0.0758					-3.0960	-3.1287	-3.1441	-3.1313	-3.1135	-3.0327	-2.8760	-2.6781

From these results it appears, that the points previously determined as the centres of force are not, in the needles C and B, precisely those in which the intensities are the greatest, but that the situations of the magnetic centres determined by the two methods very nearly agree; the distance of this centre from that of figure being, for the needle C, 1.105 inch towards the unmarked end, and for the needle B, 0.6214 towards its marked end. As might have been anticipated, the intensities of the poles were changed, as well as their situations, by the disturbance of the magnetism in either branch; and this not only in the branch to which the disturbing magnet had been applied, but also in the other. In the branch to which the magnet had been applied, the intensity was reduced in about the ratio of 83 to 31 in the needle B, and of 83 to 35 in the needle C; and in the other branch, in the ratio of 83 to 43 in B, and of 83 to 52 in C. In both cases the least intense pole was much more diffused than the other; the intensity in the branch to which the magnet had been applied changing but little over a space of nearly two inches. It appears likewise, that at the points previously determined as the centres of force in the two branches of each needle, the intensity multiplied by the distance from the magnetic centre is nearly the same on each side of that centre; these products on the contrary sides being 8.9 and 9.2 with the needle B, and with C, 8.7 and 8.8.

The observations on the deviations of the three needles, A, B, C, with their magnetism thus distributed, produced by the 18-inch shell, are contained in the following Table: they were made in precisely the same manner as those already described, with the needles A, D, P; the deviation of one needle having been observed for any azimuth, it was removed from the pivot and replaced by another, and this again by the third without moving the pivot after it had been adjusted to the required position: the vertical and horizontal distances of the shell were likewise the same as in the former case.

Needle of which the deviation was observed.	Azimuth of the Shell's centre from North.	The centre of the Shell 16.8 inches from the Needle's centre, and											
		10 inches below the horizontal plane passing through that centre.				In the horizontal plane passing through that centre.				10 inches above the horizontal plane passing through that centre.			
		Azimuth of the Shell's centre.		Means of the deviations with the Shell West and with the Shell East.		Azimuth of the Shell's centre.		Means of the deviations with the Shell West and with the Shell East.		Azimuth of the Shell's centre.		Means of the deviations with the Shell West and with the Shell East.	
		West.	East.	Deviations.	Deviations.	West.	East.	Deviations.	Deviations.	West.	East.	Deviations.	Deviations.
C, having had its magnetic medium disturbed in the marked or northern branch.	20	9 13W	8 57E	+ 9 05 00	5 38W	5 20E	+ 5 29 00	7 06E	6 42W	- 6 54 00			
	40	17 07	17 02	17 04 30	9 03	8 54	8 58 30	14 33	13 57	14 15 00			
	60	23 57	23 37	23 47 00	9 14	8 33	8 53 30	24 26	24 05	24 15 30			
	80	27 48	28 04	27 56 00	4 56	4 27	4 42 30	55 36	35 20	35 28 00			
	100	29 34	29 50	29 42 00	2 41E	3 05W	- 2 53 00	38 28	38 33	38 30 30			
	120	29 30	29 26	29 28 00	12 22	12 47	12 34 30	32 56	33 07	33 01 30			
	140	27 27	27 08	27 17 30	14 45	15 15	15 00 00	23 17	23 51	23 34 30			
	160	21 21	21 19	21 20 00	8 54	9 22	9 08 00	11 46	12 23	12 04 30			
A, having its magnetic medium similarly distributed in the two branches.	20	10 50	10 36	10 43 00	7 18W	7 05E	+ 7 11 30	12 53	12 22	12 37 30			
	40	20 30	20 18	20 24 00	11 38	11 36	11 37 00	21 18	20 46	21 02 00			
	60	28 24	28 09	28 16 30	10 30	10 04	10 17 00	27 39	27 30	27 34 30			
	80	32 22	32 47	32 34 30	3 43	3 28	3 35 30	32 12	31 57	32 04 30			
	100	31 54	32 32	32 13 00	4 28E	4 44W	- 4 26 00	32 28	32 23	32 30 30			
	120	27 54	28 10	28 02 00	11 20	11 35	11 27 30	28 09	28 02	28 05 30			
	140	21 48	21 50	21 49 00	12 20	12 38	12 29 00	30 10	20 31	20 20 30			
	160	13 18	13 10	13 14 00	7 30	7 48	7 39 00	10 18	10 41	10 29 30			
B, having had its magnetic medium disturbed in the unmarked or southern branch.	20	11 40	11 32	11 36 00	7 53W	7 50E	+ 7 51 30	17 58	17 12	17 35 00			
	40	22 06	22 03	22 04 30	12 30	12 35	12 32 30	24 53	24 24	24 38 30			
	60	30 50	30 37	30 43 30	10 33	10 12	10 22 30	28 37	28 25	28 31 00			
	80	34 52	35 32	35 12 00	2 42	2 31	2 36 30	30 30	30 12	30 21 00			
	100	33 06	33 56	33 31 00	4 58E	5 04W	- 5 01 00	39 45	29 23	29 34 00			
	120	26 24	26 52	26 38 00	10 20	10 23	10 21 30	25 24	25 15	25 19 30			
	140	19 00	19 08	19 04 00	11 00	11 10	11 05 00	18 26	18 34	18 30 00			
	160	10 40	10 36	10 38 00	6 46	6 54	6 50 00	9 22	9 41	9 31 30			

The deviations of A in this Table, are the values of ϕ in the equations (10) and (11): the deviations of C and B are the corresponding values of ϕ , the value of k being plus in the first case, and minus in the second. To compare these equations with the observations, it is necessary that the value of p should be known as well as that of k . This may be determined from the equations by means of the observed values of ϕ , and ϕ in the above table; but the computations from the equation (10) would be so laborious, that I must acknowledge I have not, for this reason, entered upon them. I have however computed the values of p from the equation (12), when the centre of the shell was in the same horizontal plane as that of the needle, although these computa-

tions are sufficiently tedious: and from the mean of the values thus determined, I have likewise computed the values of ϕ , or the deviations of A, both from the observations with C and from those with B, by means of the equation (11), taking $k = 1.105$ in the one case, and $k = -0.6214$ in the other, as determined from the times of vibration of the Needle I.

The following TABLE contains the results thus obtained.

Azimuth of the Shell's centre from north, or value of ϕ .	The deviations of A deduced from those of C.					The deviations of A deduced from those of B.				
	The deviations of A deduced from those of C.					The deviations of A deduced from those of B.				
	Observed values of ϕ .	Observed values of ϕ .	Values of p , computed from the equation (12), making $k = 1.105$.	Values of ϕ , computed from the equation (11), making $p = 1.0782$.	Difference between the observed and computed values of ϕ .	Observed values of ϕ .	Observed values of ϕ .	Values of p , computed from the equation (12), making $k = -0.6214$.	Values of ϕ , computed from the equation (11), making $p = 1.0551$.	Differences between the observed and computed values of ϕ .
20	20 29	2 11 $\frac{1}{2}$	1.1256	26 57	-14 $\frac{1}{2}$	2 51 $\frac{1}{2}$	2 11 $\frac{1}{2}$	1.0289	2 20	+06 $\frac{1}{2}$
40	8 58 $\frac{1}{2}$	11 37	1.1180	11 15	-22	12 32 $\frac{1}{2}$	11 37	1.0314	11 51	+14
60	8 53 $\frac{1}{2}$	10 17	1.0654	10 25	+08	10 22 $\frac{1}{2}$	10 17	1.0433	10 24	+07
80	4 42 $\frac{1}{2}$	3 35 $\frac{1}{2}$	1.0617	3 39	+03 $\frac{1}{2}$	2 36 $\frac{1}{2}$	3 35 $\frac{1}{2}$	1.1617	3 14	-21 $\frac{1}{2}$
100	2 53	4 36	1.2045	4 03	-33	5 01	4 36	1.0781	4 29	-07
120	12 34 $\frac{1}{2}$	11 27 $\frac{1}{2}$	1.0116	12 13	+45 $\frac{1}{2}$	10 21 $\frac{1}{2}$	11 27 $\frac{1}{2}$	1.0489	11 30	+02 $\frac{1}{2}$
140	15 00	12 29	1.0095	13 12	+43	11 05	12 29	1.0306	12 44	+15
160	9 08	7 39	1.0294	7 55	+16	6 50	7 39	1.0176	7 52	+13
Mean value of p			1.0782			Mean value of p			1.0551	

The deviations of A computed from those of B, agree with the observed deviations of A quite as nearly as we can expect; but those computed from the deviations of C do not approximate so closely to the observations. The difference in the values of ϕ and ϕ principally arises from the value of k , and any error in determining this will have a corresponding effect in the values of ϕ determined from those of ϕ . From the results here, the value of k for the needle B appears to have been determined with considerable accuracy; and I am not aware of any circumstances in the observations that should have rendered it less so for the needle C. It is however to be observed, that the

magnetic centre will not be precisely the same for all distances at which the needle may act, and that the position of this point will vary the more, the greater is its distance from the centre of figure of the needle. If for the needle C we increase the value of k by one-tenth, that is making it 1.2155 instead of 1.105, all but one of the computed values of ϕ will agree more closely with the observed values, and this one will still be extremely near to the corresponding observed value. This will be seen by the following Table computed as the preceding, excepting that, in taking the mean of the values of p , I omit that corresponding to $\theta = 100^\circ$, and which I have done because a very small error of adjustment, whether arising from the compass not being placed exactly in the required position, or from the centre of the shell not occupying accurately the centre of the circle to which the compass was adjusted, would here have a most sensible effect upon the deviations; and such appears to have been the case, from the difference in the observed values of ϕ , when θ was 80° , and when θ was 100° .

The centre of the Shell in the same horizontal plane as that of the Needle.					
Azimuth of the Shell's centre from north, or value of θ .	The deviations of A deduced from those of C.				
	Observed values of ϕ .	Observed values of ϕ .	Values of p , computed from the equation (12), making $k = 1.2155$.	Values of ϕ , computed from the equation (11), making $p = 1.069$.	Difference between the observed and computed values of ϕ .
20	5 29	7 11½	1.1037	7 01	-16½
40	8 58½	11 37	1.0996	11 20	-17
60	8 53½	10 17	1.0570	10 24	+07
80	4 42½	3 35½	1.1031	3 28	-07½
100	2 53	4 36	(1.1807)	4 05	-32
120	12 34½	11 27½	1.0224	11 59	+32
140	15 00	12 29	1.0354	12 49	+20
160	9 08	7 39	1.0617	7 41	+02
Mean value of p			1.0690		

Considering that the equation (6), from which (11) is derived, does not

afford a close approximation at this distance of the shell when the length of the needle is six inches, as appears from the observations with the needles A, D, P; and that in deducing this equation, the approximation was limited to the first power of $\frac{k}{R}$, I scarcely anticipated so close an agreement between the observed deviations of the undeteriorated needle A, and the deviations of such a needle computed from the observed deviations of the deteriorated needles B and C: this agreement has therefore fully confirmed the views which I originally took of this subject, as stated in a note to my paper in the Transactions of last year. There can be no doubt that the equation (11) affords the proper correction by which to obtain the values of ϕ from those of ϕ , when the centre of the shell is in the same horizontal plane as that of the needle; and the equation (10) will give the same correction when its centre is not in that plane. I have given the observations requisite for comparison; but this comparison I must leave to others who may be interested in the inquiry, and have more leisure for these tedious computations than the duties of my situation have allowed me.

Instead then of these "secondary deflections," as they have been termed, due to the deterioration of the needles, being inconsistent with the hypothesis "which attributes the deflection of a magnetized needle to the general central attraction of the iron on an imaginary needle passing through the pivot (centre) in the line of the dip," as is assumed in the paper giving an account of experiments with deteriorated needles, I have shown I think clearly, not only that they are consistent with it, but that this hypothesis affords the proper corrections to observations with such imperfect instruments; and therefore additional weight is given to the arguments which might have been previously adduced in support of this hypothesis. Should there be any still disposed to think that the hypothesis of induced magnetism will give closer approximations to the observed deviations than I have derived from that of a central action of the iron, the determinations which I have given of the distribution of the magnetism in these needles, will enable them to compute the deviations on that hypothesis, and to exhibit the resulting approximations.

I have, however, no intention of controverting the hypothesis of induced magnetism. That hypothesis undoubtedly affords satisfactory explanations of

many magnetical phenomena; but there is one experiment, and that a very decisive one, which appears quite inconsistent with it; and until that experiment can be clearly explained on the hypothesis, that iron acts by induced magnetism alone, I think some reservation is necessary in our assent to that hypothesis. According to it, the lower part of a bar of steel or iron is a south pole, and the upper a north pole; and when the ends of the bar are reversed, the poles are immediately so likewise. We might admit such change of the poles in the case of soft steel or iron, but by no means in that of hard steel. The following experiment of Mr. BARLOW's requires the same admission in both cases: "I procured a steel bar three feet long, an inch broad, and half an inch thick, and had it rendered, according to the expression of the workmen, "as hard as fire and water could make it;" and I must say that I was not at all surprised to find that it produced precisely the same effect as the softest iron, changing its poles with its position (to adopt the language of our author, Biot,) with equal facility*;" that is, I conceive, the deviations of a needle corresponding to such change. As the rapid change in the poles, which is here necessary to suppose, on the hypothesis of induction from the earth, is quite at variance with the phenomena observed on the approach of a magnet to a bar of hard steel, and its subsequent removal, it is necessary that the cause of this difference should be clearly shown, before we admit that iron or steel has no action upon a needle, but that which arises from the separation of its magnetism by the influence of the earth or of the needle. When a mass of iron is so far removed from a magnetized needle that the magnetism induced in it from the needle produces but little effect, I have shown that the deviations computed on the hypothesis of a central attraction, agree with those observed, even in cases which were supposed directly opposed to such an hypothesis; and therefore it is not necessary to suppose that the iron is polarized by the action of the earth.

However, whether we admit the hypothesis of a central attraction independent of the action by induction when the iron is in the immediate neighbourhood of a magnet, or not, this hypothesis has the advantage of very readily connecting all the phenomena observed in the deviations of a mag-

* Magnetic Attractions, p. 124, 1st edition: the experiment is omitted in the 2nd edition.

netized needle which are due to the action of iron; of indicating immediately the nature of the deviations, even under very complicated circumstances, as I have shown in the case of the deteriorated needles; and still further of affording ready and close approximations to numerical results in all positions within certain limits of distance from the attracting body.

S. H. CHRISTIE.

*Royal Military Academy,
22nd May, 1828.*

XVIII. *Description of a sounding board in Attercliffe Church, invented by the*
Rev. JOHN BLACKBURN, Minister of Attercliffe-cum-Darnall, Sheffield.

Read June 5, 1828.

THE material is pine wood. The surface is concave, and is generated by half a revolution of one branch of a parabola on its axis.

The distance from the focus to the vertex = 2 feet.

The length of the abscissa is . . . = 4 feet.

The length of the ordinate to the axis . . = $\sqrt{32}$ feet.

= nearly 5.7

= rad. of outer circle.

The axis is inclined forwards to the plane of the floor at an angle of about 10 or 15 degrees, and elevated so as that the speaker's mouth may be in the focus.

A small curvilinear section is taken away on each side from beneath, that the view of the preacher from the north and south galleries may not be intercepted; whence the outer semicircle is imperfect.

This, however, gives an appearance that is not inelegant; and the outer edge being ornamented with crockets and leaves and with a finial at the highest point, and the concave surface being painted in imitation of a groined oak canopy, the effect of the whole is pleasing to the eye.

A curtain is suspended from the lower edge of the canopy for about 18 inches on each side.

1. By means of this erection the volume of sound is increased in a very considerable ratio (perhaps as 5:1), and is thrown powerfully, as well as distinctly, to the most distant parts of the church; so that whereas formerly the difficulty of hearing an intelligible sound was very great, now that difficulty is effectually removed. e. g.

The preacher was scarcely audible even in the pews near the pulpit, and not at all in those more remote: he may now be heard in every part, and nowhere more distinctly than in the west gallery, or under it, on the ground-floor.

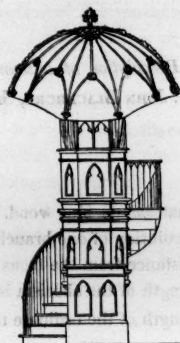
2. It should seem that the voice is reflected in a direction parallel to the axis; for let A stand in the pulpit, and B stand first in the west gallery opposite to the pulpit, and then in the side galleries; though B is much nearer to A in the latter case than in the former, he can yet hear with decided advantage when opposite to A (i. e. at the greater distance from him).

The side galleries appear to be benefited rather by the increased volume of sound, and by the secondary vibrations excited in a lateral direction.

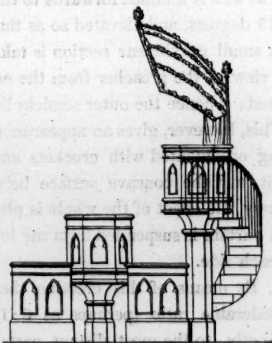
3. It appears also that vibrations proceeding from a distant point and moving in the direction of the axis, are reflected from the parabolic surface towards the focus. For let A stand in the pulpit as before, and B in a distant point opposite to A, A can then converse with B in a whisper; whilst C, standing at an intermediate point, cannot at all distinguish the words spoken by B; he can however hear what is said by A. Also if B, at a distance, opposite to the sounding board, speaks; whilst A places one ear in the focus of the parabola and one ear towards B, the effect produced is that of a voice close to the ear, and in a direction the reverse of that from which it really proceeds.

4. The converse of this also appears true from the following experiment.

Front View.



Side View.



Let B remain in the situation last supposed, and let A place his face towards the parabolic surface, and his back towards B; let A now speak, having his mouth in the situation of the focus, and he will be heard as distinctly as when his face was turned towards B.

5. If the mouth of the speaker is placed much within or without, above or below the focus, the effect is proportionably diminished.

6. The distinctness of articulation preserved may perhaps in some measure be accounted for thus. Assuming that the sound issuing from the focus is reflected in a direction parallel to the axis; assuming also that the velocity of sound is uniform; then the vibrations of the air proceeding from the focus and striking the parabolic surface, at whatever point, will arrive at the same moment of time at a plane perpendicular to the axis. For (according to the properties of the parabola) the sums of the distances (from the focus to the paraboloid, and from the paraboloid to the plane so situated) are always equal to each other: it must however be admitted, that the velocity of sound is too great to allow much dependence to be placed on this conclusion; but it is here proved beyond dispute, that a parabolic surface is capable of being successfully applied to the purpose of a sounding board; whether other concave surfaces similarly situated would be equally successful, or other materials better adapted to answer the end than pine, it might be worth while by experiment to ascertain.

7. Whilst the figure of the canopy remained perfect, the effect was most complete; perhaps it might be improved if constructed larger, or in other words, if continued further; or (should circumstances permit) if the parabola were to perform an entire revolution round its axis; but the distance from the focus to the vertex (which regulates the curve) must depend on the supposed situation of the speaker, which will vary according to the diameter of the pulpit.

JOHN BLACKBURN.

Attercliffe Parsonage, near Sheffield,

May 23, 1828.

Let B remain in the situation just supposed, and let A have his feet towards the parallel surface, and his feet towards B. In A's new position, his head is in the situation of the head, and he will be in the same position as when his feet were towards B. In A's new position, his head is in the same position as when his feet were towards B. In A's new position, his head is in the same position as when his feet were towards B.

Let B remain in the situation just supposed, and let A have his feet towards the parallel surface, and his feet towards B. In A's new position, his head is in the situation of the head, and he will be in the same position as when his feet were towards B. In A's new position, his head is in the same position as when his feet were towards B. In A's new position, his head is in the same position as when his feet were towards B.

Let B remain in the situation just supposed, and let A have his feet towards the parallel surface, and his feet towards B. In A's new position, his head is in the situation of the head, and he will be in the same position as when his feet were towards B. In A's new position, his head is in the same position as when his feet were towards B. In A's new position, his head is in the same position as when his feet were towards B.

XIX. On the mutual action of sulphuric acid and alcohol, and on the nature of the process by which ether is formed. By HENRY HENNELL, Esq. Communicated by WILLIAM THOMAS BRANDE, Esq. F.R.S.

Read June 19, 1828.

1. I WAS some time since engaged in an investigation of the nature of oil of wine and of the salts called sulphovimates: the results I obtained were considered of sufficient importance to be honoured with a place in the Philosophical Transactions*. The oil of wine and sulphovinic acid are substances produced during the mutual action of sulphuric acid and alcohol in the well-known process adopted for the preparation of ether; and an important point with me, during the above investigations and since that time, has been to develop the particular changes which take place when ether is formed from sulphuric acid and alcohol. I perceive by the *Annales de Chimie* for November last, that MM. DUMAS and BOULLAY have been engaged on the same subject, and have experimented on and considered, not only the formation of ether, but also the nature of sulphovimates, and, as they supposed, though incorrectly, of oil of wine†. That our results with regard to sulphovimates and oil of wine differ, may be seen from the published accounts; and there is not less difference between their conclusions with regard to etherification, and the results I have obtained, which I have now to describe.

2. When alcohol and sulphuric acid in equal weights are put together without the application of any heat beyond that generated during the mixture, the most abundant and important product is sulphovinic acid, above one half of the sulphuric acid being converted into that peculiar acid by union with hydro-carbon‡. But when such a mixture containing so large a proportion

* Phil. Trans. 1826. Part 3rd.

† The substance which these gentlemen operated upon appears, from their own account of its preparation, to have been the hydro-carbon separable from oil of wine by the action of alkalies, and not that peculiar substance which has hitherto been called oil of wine.

‡ The sulphuric acid loses half its saturating power by the union, and all the salts formed by the new acid are soluble.

of sulphovinic acid is distilled, the most important product is a new substance, namely ether, and the sulphovinic acid disappears. The questions which then arose were, whether the ether was formed altogether from the direct action of the remaining alcohol and sulphuric acid in the mixture, or whether the sulphovinic acid might not also assist, or whether it might not be an essential state of the elements intermediate between the mixture of the acid and alcohol and the development of the perfectly formed ether. MM. DUMAS and BOULLAY, who have considered the same questions, or at least some of them,—decide, that the portions of materials which form ether, are altogether independent of those which produce sulphovinic acid: but the following facts prove in my opinion the contrary of this conclusion.

3. A portion of oil of vitriol was selected for some comparative experiments, and also some alcohol of specific gravity 0.820: five hundred grains of the oil of vitriol precipitated by acetate of lead, gave 1500 grains of sulphate of lead.

4. Five hundred grains of the oil of vitriol were mixed with five hundred grains of the alcohol, and after forty-eight hours, diluted and precipitated by acetate of lead; only 616 grains of sulphate of lead were produced; so that very nearly three-fifths of the sulphuric acid had become sulphovinic acid by the effect of mixture, and little more than two-fifths remained to act as sulphuric acid upon the remaining alcohol, full two-thirds of the quantity employed.

5. Another mixture of acid and alcohol in the same proportions, and made at the same time as the above, was then distilled until 117 grains had passed over, consisting of water, alcohol, and a portion of ether. The residue in the retort had not undergone any charring effect; and being diluted, was precipitated by the acetate of lead: the quantity of sulphate of lead obtained, amounted to 804 grains, indicating an increase in the quantity of sulphuric acid equivalent to 188 grains of sulphate of lead.

6. A similar mixture of alcohol and sulphuric acid, made at the time and in the same proportions as the two former, was then distilled until two hundred grains had been received, the greater part of which was ether; the uncharred residual matter in the retort being then diluted, was precipitated by acetate of lead as before; 986 grains of sulphate of lead were obtained. This contained

nearly two-thirds of the sulphuric acid first added, and the increase by distillation had been much more than one-half of that which existed before the application of heat: so that during the distillation, and simultaneously with the formation of ether, a quantity of sulphovinic acid had been re-converted into sulphuric acid, and the latter appeared to increase in quantity in proportion to the increase of ether in the distilled products.

7. A similar mixture of alcohol and acid, made at the same time and in the same proportions as the three former, was then distilled until two hundred grains had passed over. Two hundred grains of water were added to the contents of the retort; 160 grains were distilled off; a second addition of two hundred grains of water was made, and the distillation continued: a further addition of five hundred grains of water was made, and the operation continued until as much product had been separated as equalled the water added;—the object was to separate all the ether and alcohol possible, for the purpose of ascertaining to what extent the conversion of sulphovinic acid into sulphuric could be carried. No smell of sulphurous acid was produced during the operation, nor did any charring of the contents of the retort occur; when precipitated by acetate of lead, 1480 grains of sulphate of lead were obtained. This is very little short of the 1500 given by the acid when unacted upon by alcohol, and shows that nearly the whole of the sulphovinic acid had been changed back into the state of sulphuric acid; and is completely at variance with the opinion, that when sulphuric acid and alcohol act upon each other, hypo-sulphuric acid is formed.

8. From these experiments it appeared probable that the ether was the product of the decomposition of the sulphovinic acid: but a mixture of equal weights of alcohol and sulphuric acid contains, besides the sulphovinic acid, a considerable quantity of unaltered acid and alcohol; for in such a mixture three-fifths (4) of the sulphuric acid would be converted into sulphovinic acid by combination with the hydro-carbon of less than one-third of the alcohol employed. I next proceeded to ascertain, whether, when no alcohol was present, ether would be produced. A quantity of the sulphovinate of potash was therefore prepared. The composition of this salt has been given in the paper in the Philosophical Transactions before referred to, and one hundred parts contain 28.84 of potash. Five hundred grains were mixed with 150 grains of sulphuric

acid, being nearly the equivalent of the potash in the salt, and then heat applied. The experiment therefore may be considered as the distillation of sulphovinic acid mixed with sulphate of potash, which it may be presumed remained inert during the process, and also with the water of the acid and of the salt. The proportion of water, it is found, has an important influence; but in the present experiment about a drachm of fluid distilled over, and left a blackened and acid salt in the retort, having the smell of sulphurous acid. A few grains of carbonate of potash being added to the distilled product, abstracted a little water: the clear decanted liquor was then mixed with a little dry muriate of lime, and by agitation separated into two portions; the upper one being decanted, amounted to nearly half a drachm, and was found to be pure ether. This result proves that ether may be formed from a sulphovinate or sulphovinic acid when no alcohol is present.

9. An experiment similar to the last in the nature and proportions of the substances used, was made, except that the sulphovinate was dissolved in its own weight of water previous to the addition of the sulphuric acid. The experiment is one therefore of the distillation of dilute sulphovinous acid, in place of that which is concentrated. The distilled product had no smell of ether, nor could any be discovered in it. About nine fluid drachms were obtained; to these, carbonate of potash was added, which separated the water, and left three drachms of a supernatant liquid, appearing by taste, smell and flame, to be alcohol: this was decanted, and poured upon muriate of lime; no ether separated, but the whole formed one solution; being distilled from the muriate it was evidently alcohol; and being mixed with its weight of sulphuric acid, gave sulphuric ether or sulphovinic acid again.

In this experiment there was no charring of the contents of the retort; and by precipitation by acetate of lead, the whole of the sulphuric acid was obtained;—not only the portion added to decompose the salt, but the double portion evolved from the sulphovinic acid upon the separation and re-arrangement of the hydrocarbon.

10. In the former paper it was shown that oil of wine when heated in water is resolved into hydrocarbon and sulphovinic acid: an experiment was therefore made upon it. Two hundred grains of oil of wine were placed in a retort, a little water added, and heat applied: about a drachm was received, which being

redistilled from carbonate of potash the product appeared to be principally alcohol, but the presence of ether was very evident.—This experiment proves the formation of ether from sulphovinic acid when no sulphuric acid was present as such at the commencement of the distillation.

With regard to the questions at the commencement of this paper, it appears to me from the facts detailed, that in the usual process for obtaining ether, the ether is not formed altogether from the direct action of the alcohol and sulphuric acid considered independently of the sulphovinic acid present; for the quantity of free sulphuric acid is small compared to the quantity of alcohol present, two-fifths only of the acid remaining, while of the alcohol more than two-thirds remain; and further, sulphovinic acid alone is readily converted into ether and sulphuric acid, (see 8.) and during the distillation of ether in the ordinary way the sulphovinic acid is always re-converted more or less completely into sulphuric acid (4. 5. 6.) it probably therefore assists much in the process. With regard to the third question, the opinion may be supported that the formation of sulphovinic acid is a necessary and intermediate step to the production of ether from alcohol and sulphuric acid; and although I do not mean to assert this view, yet it deserves a few remarks.

In no manner which has yet been devised can ether be formed from alcohol and sulphuric acid without the presence of sulphovinic acid. Whenever ether has been formed, sulphovinic acid has been present; whenever the sulphuric acid is diluted so far as not to form sulphovinic acid with alcohol, it also refuses to form ether with alcohol. Sulphovinic acid will produce ether without the assistance of alcohol. And although the ether produced when a mixture of equal weights of alcohol and sulphuric acid are distilled, appears to be in greater quantity than can arise from the decomposition of the sulphovinic acid existing in the mixture previous to the action of heat, it is not I think inconsistent to suppose, that at the same time that one portion of sulphovinic acid is resolved into sulphuric acid and ether, another may be formed from alcohol and sulphuric acid; and that sulphovinic acid is formed in a mixture of sulphuric acid and alcohol by heat, is proved by the following experiment. Five hundred grains of oil of vitriol were diluted by five hundred grains of water; when cold, to the dilute acid was added two thousand grains of alcohol, specific gravity 0.820. The following day this mixture was examined for sul-

phovinic acid, but none had been formed: it was placed in a retort, and a quantity distilled off nearly equal to the weight of the alcohol employed: this had a specific gravity of 0.842. Carbonate of potash separated a considerable portion of water, the original alcohol would not even moisten that salt; the residue in the retort was examined, and now sulphovinic acid was found; the evidence of which was, carbonate of lead being dissolved in considerable quantity; here sulphovinic acid had been formed by heat, where it did not previously exist. This result appears also opposed to the opinion, that in the formation of ether the sulphuric acid acts simply by abstracting water from the alcohol; for the dilute acid here gave up a portion of its water during the distillation, and separated from the alcohol a portion of hydro-carbon.

It has already been shown (9.) that the production of ether is materially influenced by the quantity of water present, and that the same sulphovinic acid will yield either ether or alcohol, as it is in a concentrated or dilute state. The hydro-carbon which, as was shown in the former paper, has the extraordinary power in oil of wine of neutralizing the whole of the acid properties of sulphuric acid, and in sulphovinic acid of neutralizing the half of them, being in the latter body in so peculiar a condition that it will unite either with that proportion of water necessary to form ether, or with the larger proportion requisite to form alcohol, according to circumstances.

In the experiments (8. 9.), in the production by distillation of ether or alcohol from sulphovinic acid more or less diluted, it appeared that sulphovinic acid might easily have its proximate elements separated and restored to their original state of sulphuric acid and alcohol. The following experiment was made with a view to illustrate this point. Five hundred grains of acid and five hundred grains of alcohol were mixed as before, and left for several days: by previous experiment it is known that more than half the sulphuric acid in this way becomes sulphovinic acid (4). By distillation and dilution at proper periods this would have given ether and alcohol, and nearly the whole of the sulphuric acid (7.): but instead of doing this, it was mixed with one thousand grains of water, and then distilled until 1400 grains had passed over. No charring or decomposition of the sulphuric acid took place; no ether was formed; but nearly the whole of the original alcohol and sulphuric acid were recovered. It may be a question whether the production of alcohol and ether

in those and similar experiments is altogether determined by the proportion of water present, or whether the difference of temperature consequent upon its variation may not have an effect.

When ether and sulphuric acid are heated together, oil of wine and sulphovinic acid are amongst the products obtained; and as this sulphovinic acid is readily converted when diluted into alcohol and sulphuric acid, so it affords a method of converting ether into alcohol: thus ether may be formed from alcohol, and alcohol from ether at pleasure, by throwing the hydro-carbon of these bodies into that peculiar state which it assumes when combined with sulphuric acid in sulphovinic acid. We may even proceed beyond this, and form either alcohol or ether, using olefiant gas as the hydro-carbon base: for I have shown in my last paper, that olefiant gas by combining with sulphuric acid, forms sulphovinic acid, and the acid so produced forms either ether or alcohol, according to circumstances which are under perfect command.

It can hardly be necessary to refer to the extraordinary remark at the end of MM. DUMAS and BOULLAY's second paper, except to state that it is singularly at variance with the facts and opinions given throughout the former part of that and the preceding paper by the same authors. Those persons who read both papers, and also those of Mr. FARADAY and myself, which were published long before the appearance of the former, will be able to decide without further comment from whom the particular views contained in those papers first emanated.

H. HENNELL.

Apothecaries' Hall.

XX. *Experiments and observations on electric conduction.* By WILLIAM RITCHIE,
A.M. F.R.S. Rector of Tain Academy.

Read June 19, 1828.

THAT some substances conduct or convey the electric fluid to a distance better than others, is a fact known to the earliest electricians; but on what power or property of the body this superiority depends, is a question on which different opinions still seem to prevail. We constantly hear the expressions "electricity is attracted by metals; the lightning is attracted by the metallic points of a conducting rod," and other expressions of similar import,—all signifying that a powerful attraction does exist between metals and the electric fluid. Now the contrary is really the fact, those bodies being the best conductors which have the least attraction for the electric fluid. From the profound mathematical investigations of M. Poisson, and the luminous writings of M. Biot, it appears that these philosophers consider the metals merely as forming the passive interior of a vessel, of which the exterior surface is the ambient air; and that the electric fluid rushes along between the atmospheric boundary and the surface of the metal, where it finds an easy passage. We are therefore to consider the metals as quite passive in the conduction of the electric fluid, and that the prime mover is the repulsive energy existing between similar atoms of the compound electric fluid. When a metallic ball connected with the earth is placed near the prime conductor, the vitreous electricity surrounding the conductor repels the vitreous electricity of the ball, and forces it to glide along to a greater distance, whilst the ball will now be surrounded by a thin film of the resinous fluid. The vitreous electricity of the conductor thus finding an easier passage in the direction of the ball, and being in a high state of tension, will, like every other elastic fluid, glide along in the direction of the ball as if it had actually been attracted by that body. The reason why it does not strike off with equal facility to a vitreous body is, not because it is less attracted by that body, but simply because it is unable to decompose with

the same facility the natural electricity belonging to the glass, on account of the powerful attraction existing between the atoms of the glass and those of the electric fluid. If the glass be thin and a metallic conductor placed in its interior, the vitreous electricity will act through the glass, decompose the fluid in the metallic conductor, and then actually strike through the glass in the direction of the metal where the resistance is least.

EXP. I. On the ends of two thermometer tubes I blew two balls of extreme tenuity. I then introduced two pieces of brass wire into the tubes till the ends reached within a small distance of the interior surface of the balls. Having brought the other ends of the tubes together, I joined them at the flame of the blowpipe, so that I had now a metallic conductor completely surrounded with glass. This being placed on a stand, and one of the balls brought near the prime conductor, I found I could take sparks, for any length of time, from the other end, in the same manner as if the glass had not been interposed. When the bulbs were about the thickness of those of a common thermometer, I observed that if sparks were taken for any length of time from the same place, they afterwards chose the same tract. I naturally concluded that the glass had been pierced, though I could not determine it by the naked eye. I found, however, that if the tubes were again separated and the air partially expelled from one of the balls by heat, and the open end of the tube placed in a vessel containing mercury, the mercury rose in the tube, but after a short time it again sunk to its proper level; clearly showing that the bulb had been pierced, though the aperture was extremely minute. I now began to suspect that in every case in which glass seemed to have been freely permeated by the electric fluid, that the fluid had been either silently conducted through it, or that, if carefully examined, it would have been found to have forced out some of the atoms of the glass. I therefore repeated the experiment with glass as thin as it could be blown without bursting, and found that the electric fluid would in that case freely permeate it; and that by no known method could I detect the smallest aperture in the glass.

EXP. II. Place an electric jar in a receiver, and partially exhaust the air; and the charge which can be given it will be very much diminished: exhaust the air still further, and it will be found impossible to communicate the slightest charge. In continuing to exhaust the air, the sparks between the ball con-

nected with the prime conductor and knob of the jar will become less and less till the electric fluid begins to flow in a continuous stream. From this experiment it is obvious that the jar ceases to receive a charge when the pressure of the air becomes equal or less than the repulsive energy existing between the atoms of similar kinds of electricity.

Exp. III. Place the Leyden phial in a receiver, to which is adapted a condensing syringe; condense the air, and the phial will receive a higher charge than in air of the ordinary density. In charging the jar, the sparks will be larger, and strike off at greater intervals than in common air.

It is quite obvious that in both of these experiments, the pressure of the air prevents the radiation of the electric fluid in the same manner as it acts in partially preventing the radiation of caloric from a heated body, or evaporation from the surface of water.

Exp. IV. Raise the end of an iron rod to a white heat, place the other end in either conductor, and the vitreous or resinous electricity will flow off to a metallic ball in a continuous stream. When the iron assumes a red heat, the current will change into a rapid succession of small sparks, which will increase in size as the iron cools. If the heated iron be presented to either conductor, the same effects will take place.

In this experiment the air surrounding the heated iron is highly rarified, and consequently exerts a diminished pressure on the electric fluid, which of course flows off by its own repulsion, exactly as it does in a receiver partially exhausted of air. The attraction of the iron for the electric fluid, when saturated with heat, will be diminished; and consequently the fluid will begin to flow off in a continuous stream, when the pressure of the air is greater than that of the air in a similar experiment in the exhausted receiver. When the iron has assumed a red heat, the surrounding air, not being so much rarified as in the case of a white heat, forces the electricity to accumulate in a small degree on the surface of the metal, and hence the commencement of the passage of the silent current into small sparks, exactly as it does in air of a certain density.

Exp. V. Raise the ends of two iron rods to a white heat, place them in the same line, with their heated ends at a considerable distance from each other; connect the cold ends with the opposite sides of a charged jar, and the jar will

be discharged by explosion, when the heated ends are at a much greater distance than it can be when the rods are cold. The air between the ends of the rods being partially rarified, will of course afford an easier passage to the electric current than air of the ordinary density, and hence the discharge will take place when the ends of the rods are more remote. This is exactly what takes place when cold rods are placed in a tall receiver partially exhausted.

EXP. VI. Place two metallic wires in the ends of a long pencil of flame formed by the blowpipe, connect them with the opposite sides of a charged jar, and the jar will be discharged by explosion along the flame. The spark will make its appearance at the point of the flame. The flame of a blowpipe is a hollow cone containing highly rarified air. The electric fluid will therefore glide along such a cone, exactly as it does along the interior of a hollow cone of glass partially exhausted of air. We are therefore not to regard flame as a conductor of electricity in the ordinary sense of the term; when the only part it performs in the conduction is merely that of forming a partial vacuum.

This fact is sometimes illustrated in a magnificent manner during violent irruptions of volcanic mountains. The air in the crater is highly rarified: and during a thunder-storm, the lightning is observed to dart into the hollow cone as if it were attracted by the flame, or by the mass of melted lava at the bottom of the crater; whereas the true cause of the phenomenon is found in the ready passage which the partial vacuum affords to the electric fluid driven off by its own repulsion from the charged cloud.

It is a well-known fact that imperfect conductors become tolerably good conductors when heated. Glass, for example, which is a very imperfect conductor when cold, conducts the electric fluid very readily when heated red hot. This is indeed what we might naturally expect from what we have assigned as the cause of conduction. Glass when cold has a powerful attraction for the electric fluid; it is therefore natural to expect that when charged with caloric, which is at least one of the ingredients of electricity, its attraction for that fluid would become less, and consequently afford a freer passage for the current along its surface. If the body be naturally a pretty good conductor of electricity, the ratio of its conducting power will not be so much increased by heat as in the case of a less perfect conductor. This at least was found by MARIANINI to be the case with fluid conductors; and from some experiments

which I have lately performed, I am led to believe that it will be found to be the case with every kind of conductor. Sir H. DAVY in some of his experiments on the conducting powers of metals, was led to conclude that the conducting powers of metals are diminished by heat, at least for voltaic electricity. It is perhaps true, that if a slender wire be heated in the middle by a spirit-lamp, the same quantity of the electric fluid will not find its way to the other end of the wire, as happens when the whole is cold. But unless the quantity of the electric fluid which escapes by the rarified portion of air surrounding the heated part be ascertained, no conclusion can be drawn with regard to the increase or diminution of the conducting powers of the wire.

In my experiments on the conducting powers of hot and cold iron, I very soon relinquished the use of wires for that of rods about half an inch in diameter.

EXP. VII. Let an iron rod be converted into the annexed form, in which C, B are brass balls, and A drawn to a fine point. Let a glass tube be drawn out at a lamp to form a slender needle about six inches long, which is to be suspended by the middle by a fine thread of glass. Place two slender pieces of glass at each end of the horizontal needle, to prevent it having much motion. Twist the thread a little till the needle rest against the opposite supports. Place its pith ball D between B, C. Heat about a foot of A B in the middle, place the rod on an insulating support, with A near the prime conductor, and bring it rapidly to its former position. Turn the index till the attraction of B just overcomes that of C, continue to turn the machine, and the attraction of B will diminish so that the ball D will move off to C as the iron cools. From this experiment, it would seem that iron when heated is a better conductor of the electric fluid than when cold. The following experiment, which I have repeated at least ten times with uniformly the same results, will place this fact beyond the possibility of doubt.



EXP. VIII. Bring two brass balls connected with the earth near the balls B, C, (the part E being heated red hot,) till the electric fluid strike off almost equally to both; allow E to cool, and the electricity will cease striking off from B, and the whole will flow off in rapid sparks from C. The experiment may be rendered still more striking, by making the whole electricity at first

flow from B: as the iron cools, sparks will begin to appear between C and its ball, and in a short time the current will cease entirely from B, and the whole will now strike off from C.

As the ball B was at a considerable distance from E, its temperature remained uniform: we are therefore led to the conclusion that the electric fluid finds an easier passage along hot than cold iron; or, to use the ordinary language of the science, iron when red hot is a better conductor of electricity than when cold.

Whether the cause of this be found in the diminished attraction which hot iron has for the electric fluid, in the diminished pressure of the ambient air, or in both causes combined, is a question which has not yet been solved; but whatever be the cause, the fact is certain, though at complete variance with our preconceived notions on the subject, and even with the results of former experiments.

EXP. IX. Suspend a magnetic needle by the glass thread as in Exp. VII. Place a rod of iron heated white hot opposite either end of the needle, apply either pole of a horseshoe magnet to the other end of the iron, and the deviation of the needle both by attraction and repulsion will be found to be greatest when the rod reaches a red heat, and will continue to diminish as the rod cools. From this experiment it is obvious that iron at a red heat conducts the magnetic influence better than when cold. Though this experiment bears some resemblance to those of Mr. BARLOW, yet, as far as I know, the fact has not been previously observed; and it affords us another striking analogy between the electric and magnetic influences.

XXI. *On magnetic influence in the solar rays.* By SAMUEL HUNTER

CHRISTIE, Esq. M.A. F.R.S. &c.

Read June 19, 1828.

THE facts which I communicated in my former paper on this subject appeared so inexplicable on any known principle, that I am induced to present my subsequent observations to the Society, although I have not succeeded in ascertaining the causes of the singular effects which I have observed. From the experiments described in that paper, it appeared that a magnetized needle, when vibrated exposed to the sun's rays, will come to rest sooner than when screened from their influence: that a similar effect is produced on a needle of glass or of copper; but that the effect upon the magnetized needle greatly exceeds that upon either of the others. To the experiments from which this was inferred, it might be objected, that the magnetized needle and the other metallic needle were not of the same weight, and that the effect upon an unmagnetized steel needle had not been compared with that upon a similar needle magnetized. I therefore, on the first opportunity, made these experiments in the most unexceptionable manner, and the results most decidedly confirmed those I had previously obtained. I endeavoured likewise to ascertain the effects that would be produced by the separate rays; but, possibly owing to the inefficiency of my apparatus, I obtained no very decided results: the violet rays appeared to produce the same effect as partially screening the needle; and the red rays, the greatest effect in diminishing the arc of vibration. The observations themselves will however best point out the nature of these effects.

My first object was to compare the effects on an unmagnetized steel needle with those on a magnetized needle, under circumstances as nearly as possible the same. For this purpose I made another needle of the same form and weight, and from the same piece of clock-spring, as the magnetized needle which I had already employed. Each needle had pasteboard glued to the

under side, to render it of precisely the same weight as two other needles of copper and of glass, which I had cut of the same form for the purpose of comparing the effects upon needles of different kinds. The length of each needle is 6 inches, and the greatest breadth 1.5 inch, the boundaries being circular arcs. The needles were vibrated by means of an apparatus, described in my former paper, from which metal was scrupulously excluded; the suspending wire being the only metal within several feet of the needle. This wire was of brass, and of such diameter, that the unmagnetized needles vibrated by the force of its torsion in very nearly the same time as the magnetized needle by the directive force of the earth. The observations are contained in the following table, where the terminal arc is, in all cases, the extent to which the needle vibrated beyond zero after completing the 100th vibration; and the terminal excess is the excess of the terminal arc when the needle vibrated in the shade above that when it vibrated exposed to the sun.

Needle vibrated.	Needle vibrated screened from the Sun.				Needle vibrated exposed to the Sun.				Terminal excess. Difference of Temp.	
	Extent of 1st Vibration.	Time of making 100 Vibrations.	100th or Terminal Arc.	Therm.	Extent of 1st Vibration.	Time of making 100 Vibrations.	100th or Terminal Arc.	Therm.		
Magnetized Steel Needle. Weight = 252½ grains.	90+88	^m 6 06.2	31.5	57.5	90+88	^m 6 01.9	18.5	106		
	90+88.5	6 06.0	31.5	57.5	90+88	6 02.0	16.5	107		
	90+88	6 06.0	31.25	58.5	90+88	6 01.8	16.5	109		
	Means...	6 06.1	31.4	57.8	90+88	6 01.9	16.5	107.3	14°.9	49°.5
	Apr. 19, from 9 ^h A.M. to 9 ^h 40 ^m A.M.				April 19, from 9 ^h 53 ^m A.M. to 10 ^h 17 ^m A.M.					
Unmagnetized Steel Needle. Weight = 252½ grains.	90+92.5	5 59.6	25	65	90+91.5	5 59.4	19	121		
	90+91.5	5 59.6	26.5	65	90+90.5	5 59.4	18.5	114		
	90+91.5	5 59.8	26.75	65	90+91	5 59.4	19	115		
	Means...	5 59.7	26.1	65	90+91	5 59.4	18.8	116.7	7.3	51.7
	Apr. 21, from 23 ^m P.M. to 52 ^m P.M.				April 21, from 1 ^h 07 ^m P.M. to 1 ^h 39 ^m P.M.					

The circumstances under which the observations with the two needles were made, were as nearly the same as I could expect to have them; and the results show that the effect produced by the sun's rays on a steel needle, when vibrated exposed to their influence, is most decidedly increased when that needle is magnetized. The small differences which are to be noticed in the extent of the first arc of vibration would have little influence on the terminal arc, the

diminution in the large arcs being so much greater than in the small ones: for instance, the needle vibrating 90° beyond zero in the first vibration, in the second it would not vibrate to 88° ; whereas the 100th vibration being 26° , the 101st would be $25\frac{1}{2}^\circ$ beyond zero. This remark applies to all the observations which I have made.

In making these observations, I first noticed that time appeared to be required, in order that the full effect arising from exposure to the sun should be produced. Thus the last observation in the shade with the magnetized needle being concluded at $9^h 40^m$, the screen was immediately removed, and I commenced an observation in the sun at $9^h 43^m$, which gave the terminal arc 23° ; and the next observation in the sun, commencing at $9^h 53^m$, gave the terminal arc $16\frac{1}{2}^\circ$. With the unmagnetized needle, the last in the shade being concluded at $0^h 52^m$, I commenced an observation in the sun at $0^h 53^m$, which gave the terminal arc 22° ; the terminal arc in the next observation commencing at $1^h 07^m$ was 19° : and again the needle being screened at $1^h 39^m 45^s$, an observation in the shade, commencing at $1^h 39^m 30^s$, gave the terminal arc $24\frac{1}{2}^\circ$ instead of 26° , which I had obtained previously. I ought to mention, that during the observation with the magnetized needle in the sun to which I have referred, the power of the sun was diminished by a haze which produced a halo round it; and the extent of the terminal arc might partly be attributed to this circumstance. I have, however, observed the same effect on other occasions where no such cause operated.

In the following table, are contained similar observations, which I made with these steel needles and two others, the one of copper, and the other of glass. The four needles were of the same form and weight, and vibrated nearly in the same time; the unmagnetized steel needle, the copper needle and the glass needle being suspended by the same wire as I had used in the foregoing experiments with the unmagnetized steel needle.

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Needle vibrated.		Time of commencing Observation.	Extent of first Vibration.	Time of making 100 Vibrations.	Terminal Arc.	Thermometer. Exposed to Sun. In Shade.	Remarks.		
Glass Needle. Weight 2524 grains.	Screened from the Sun.	July 3rd. 10 02	90+88	m 6 01.0	29.3	124.5	74°	Sun shining clear.	
		10 12	90+88	6 01.0	29.0	132.0	75	Sun shining clear.	
		10 21	90+88	6 01.0	29.0	118.0	73.5	Clouds over sun.	
		Means...	90+88	6 01.0	29.17		74.2		
		10 38	90+87.5	6 01.0	23.0	123.0	74.8	Sun clear.	
	Exposed to the Sun.	0 46	90+88	6 01.5	22.0	132.0	78.5	Sun clear: dew on the glass of the instrument.	
		0 56	90+88.3	6 01.6	23.5	141.5	79.0	Sun clear: dew on the glass of the instrument.	
		Means...	90+87.9	6 01.4	22.53	132.2		Terminal excess = 8.34; { diff. of temp. in } = 58° sun and in shade. }	
		3 35	90+90	5 57.4	29.0	136	80.0	Sun clear.	
		3 45	90+89.5	5 57.2	27.5	140	80.6	Sun clear.	
Copper Needle. Weight 2524 grains.	Screened from the Sun.	3 53	90+89.3	5 57.2	28.0	141	80.4	Sun clear.	
		Means...	90+89.6	5 57.27	28.17	139			
		4 03	90+90.5	5 57.2	32.75	142	80.0	Sun clear.	
		4 12	90+91.0	5 57.2	34.0	143	80.0	Sun clear.	
		4 20	90+89.0	5 57.0	33.0	142	80.0	Sun clear.	
	Means...	90+90.2	5 57.13	33.35		80.0	Terminal excess = 8.08; { diff. of temp. in } = 59° sun and in shade. }		
	Unmagnetized Steel Needle. Weight 2524 grains.	Screened from the Sun.	July 3rd. 9 15	90+87	6 00.2	28.5	135	74.7	Sun clear.
			9 25	90+88	6 00.4	29.25	136	75.3	Sun clear.
			9 34	90+89	6 00.4	29.5	138	76.0	Sun clear.
			Means...	90+88	6 00.33	29.08		75.3	
Exposed to the Sun.		9 43	90+88	5 59.8	25.25	136	76.0	Sun clear. } These two observations are not included Sun clear. } in the mean.	
		9 52	90+89	5 59.6	23.5	141	78.0		
		10 02	90+87.5	5 59.2	22.0	147.8	79.0	Sun clear: dew on the glass of the instrument.	
		10 11	90+89.5	5 59.0	22.0	148	79.4	Sun clear: dew on the glass of the instrument.	
		10 20	90+87	5 59.0	21.0	149	80.0	Sun clear: dew on the glass of the instrument.	
Means...		90+88	5 59.07	21.67	148.3		Terminal excess = 7.31; { diff. of temp. in } = 73° sun and in shade. }		
Magnetized Steel Needle. Weight 2524 grains.	Screened from the Sun.	0 38	90+87	6 12.3	29.5	142	86	Sun clear.	
		0 48	90+88	6 12.6	29.5	140	85.7	Sun clouded for 50°.	
		1 04	90+87.5	6 12.2	29.5	129	85.4	Sun clouded.	
		Means...	90+87.5	6 12.37	29.5		85.7		
		1 16	90+88	6 12.4	21.5	141	86.2	Sun clear. Not included in the mean.	
	Exposed to the Sun.	1 26	90+88	6 12.4	17.5	149.8	87.6	Sun clear.	
		1 35	00+88.3	6 13.9	16.0	155.0	88.8	Sun clear.	
		Means...	90+88.1	6 13.15	16.75	152.4		Terminal excess = 12.75; { diff. of temp. in } = 68.7° sun and in shade. }	

This table exhibits in the clearest manner the difference between the effect on a magnetized needle and that on any other, when vibrating exposed to the influence of the sun's rays. Had the time during which the magnetized needle vibrated been $6^m 13^s$ instead of $6^m 13^s$, the terminal excess would have been $12^{\circ} 3$ nearly, supposing it to be nearly proportional to the time. However, to render the time in which this needle made one hundred vibrations more nearly equal to those of the others, I remagnetized it; and four hours afterwards made the following observations.

Needle vibrated.	Time of commencing Observation.	Extent of the first Vibration.	Time of making 100 Vibrations.	Terminal Arc.	Thermometer.		Remarks.
					Exposed to Sun.	In the Shade.	
Magnetized Steel Needle. Weight 252 grains.	Screened from the Sun.	July 24. 0 10	$90^{\circ} 88'$	$6^m 01.1^s$	29	149	84.5 Sun clear.
		0 28	$90^{\circ} + 87.75'$	$6^m 01.3^s$	29	150	84.6 Sun clear.
		0 37	$90^{\circ} + 88'$	$6^m 01.4^s$	29	149.6	83.0 Sun clear.
		Means	$90^{\circ} + 87.9'$	$6^m 01.27^s$	29		84.0
	Exposed to the Sun.	0 46	$90^{\circ} + 88'$	$6^m 02.4^s$	18	149.0	83.5 Sun clear. At $6^m 02^s.4$, terminal arc = 18°
		1 00	$90^{\circ} + 88'$	$6^m 05.8^s$	16.75	146	83.2 Sun clear. At 5 58.5 or 98^{th} vib. arc = 17.25
		1 12	$90^{\circ} + 87.75'$	$6^m 06.4^s$	16.5	147	83.0 Sun clear. At 5 59.1 or 98^{th} vib. arc = 17.0
		1 24	$90^{\circ} + 87.75'$	$6^m 06.8^s$	15.5	145	83.4 Sun clear. At 5 59.5 or 98^{th} vib. arc = 16.0
		Means	$90^{\circ} + 87.9'$	$6^m 05.35^s$	16.7	146.75	83.3 Time = $5^m 59.98^s$, arc = 17.06
	Screened from Sun.	1 33	$90^{\circ} + 87.75'$	$6^m 07.6^s$	24.5	145.0	83.8 Sun clear. { At $6^m 0.0^s$ or 98^{th} vib. arc = 24.75 Not included in the mean.
		1 46	$90^{\circ} + 87.75'$	$6^m 08.8^s$	27.5	144.0	83.8 Sun clear. At $6^m 01.4$ or 98^{th} vib. arc = 28.25
		1 55	$90^{\circ} + 87.75'$	$6^m 09.0^s$	27.75	140.8	82.8 Sun clear. At $6^m 01.6$ or 98^{th} vib. arc = 28.5
		Means	$90^{\circ} + 87.75'$	$6^m 08.9^s$	27.62		83.3 Time = $6^m 01^s.5$, arc = 28.38
	Taking the mean of the arcs in the shade, and in the sun, when the time of vibrating was the same, we have, Terminal Excess = $11^{\circ} 5$; Difference of temperature in sun and in shade = $63^{\circ} 1$.						

Some of these observations again appear to indicate, that the full effect of diminishing the terminal arc was not produced immediately on exposing the needle to the sun's rays, nor was the full effect of increasing it immediately produced on screening the needle from their influence. The first observation with the unmagnetized needle exposed to the sun, commenced immediately that the screen was removed, and the terminal arc was $25\frac{1}{2}^{\circ}$; in the next observation the terminal arc was $23\frac{1}{2}^{\circ}$; but in those which followed, it was

reduced to 22° and 21° , which appeared to be the limit. The observations with the magnetized needle give similar results. The terminal arc was $21\frac{1}{2}^{\circ}$ in the first observation with the needle exposed to the sun, which commenced immediately that the screen was removed; and this arc was only $17\frac{1}{2}^{\circ}$ and 16° in the subsequent observations. The result was similar in the first observation in the sun on the 4th of July; although the difference between the terminal arc in this and the following observations was not so considerable: but in the first observation when the needle was screened, after having been exposed to the sun, and which commenced immediately that the needle was screened, the terminal arc was only $24\frac{1}{2}^{\circ}$ instead of 29° , which it had been in the shade previous to exposure to the sun. This circumstance would appear to indicate that the diminution of the terminal arc, on exposure to the sun, was caused by the heat which it imparted; but subsequent observations clearly showed that this was not the case, and that the effect was only so far dependent on the heat of the sun, that this appeared in some instances to measure the intensity of the action which produced the diminution. These observations in July, compared with those in April, appear to indicate that the effect of the sun's rays on the magnetized needle in April was greater than in July; although their intensity, measured by the heat imparted, was much less in the former case than in the latter. If, however, such is really the case, nothing but observations carefully made and repeated during a series of years, would satisfactorily establish such a fact. It may appear that I have unnecessarily multiplied observations all pointing to the same conclusions: my object in making them was, in the first instance, to satisfy myself that the effects which I observed invariably took place under certain circumstances; and I, in all cases, give these repeated observations, that others may be enabled to draw their own conclusions from them, should they doubt the correctness of mine.

In order to determine how far the vibrations of a needle are influenced by the separate rays, I placed a glass cylinder, ground plane on the under edge, upon the plate of glass covering the compass-box; and I proposed, by having fluids of different colours in this cylinder, to transmit only particular rays to the needle. Owing, however, to the irregularities on the sides of the cylinder, so much light was lost in transmission through the cylinder even when empty, that the effect was considerably diminished; and after a few trials I gave up

this method of observing, as before I could have procured another cylinder better adapted to my purpose, I should no longer have been able to devote the requisite time to the inquiry. The only effect which I observed was, that transmitting the rays through half an inch of the sulphate of indigo very much diluted, appeared to be nearly equivalent to intercepting them altogether. Another method which I adopted for transmitting only particular rays, was by placing circles of glass painted with different transparent colours over the compass; but in this manner I obtained no very determinate results as respected the effects of rays of different colours. Although I failed in the particular object I had in view, the observations led to one important conclusion, which I have already noticed, that the diminution in the terminal arc was not produced by the heat imparted by the sun, either to the needle or the medium in which it vibrated.

The difference made in the apparatus was, that the compass-box was nine inches in diameter, to contain a graduated circle of paper eight inches in diameter; so that the needle having bristles in the direction of its axis projecting 1.5 inch beyond its extremities as indexes, I was enabled to read the direction of the needle, although it was hid by the coloured glass placed over the glass top of the instrument. In order that I might have a tolerably correct measure of the temperature within the compass-box during the different observations, I placed inside, a small spirit thermometer, graduated on ivory, and having the tube fixed to the scale by narrow bands of paper. I likewise employed a powerful lens eleven inches in diameter to concentrate the rays on the space in which the needle vibrated. I attempted also to bring the focus of the lens within the compass-box, but the heat was so great that it burned part of the apparatus; and I desisted, lest by injuring the essential parts I should put an end to my experiments. The coloured glass consisted of two semicircles, each having a small concentric semicircle cut out, so that they fitted each other round the tube carrying the suspending wire. Having clearly ascertained the fact that the effect produced on the magnetized needle was different from that on any other, the observations which I now made were on the vibrations of a magnetized needle alone; and I employed that which I had already used, removing however the additional weight of paper which had been fixed to it, so that its weight was now only 197 grains, and its time of vibration was likewise diminished. The following are the observations which I thus made.

Circumstances under which the Needle vibrated.	Time of commencing the Observation.	Extent of the first arc of Vibration.	Time of making 100 Vibrations.	Ter- minal Arc.	Thermometer. Within the Compass- box.	Exposed to the Sun.	In Shade.	Remarks.
Screened from the Sun.	July 29th. h m		m s					
	9 15	90+84.5	5 31.0	19.8	72.5	123.0	61.4	Sun clear.
	9 30	90+84.8	5 31.5	21.5	73.0	110.6	60.8	Sun clear.
	9 40	90+84.8	5 31.1	20.5	73.0	126.0	63.0	Sun clear.
Exposed to the Sun.	9 50	90+84.8	5 30.0	9.8	100.0	134.0	65.0	Sun clear. { The full effect appears to have been produced almost immediately.
	10 04	90+84.8	5 30.4	14.8	95.5	104.0	64.6	Sun obscured by dark clouds.
	10 15	90+84.8	5 29.8	9.3	102.9	130.0	64.8	Sun clear.
	10 23	90+84.8	5 31.1	14.3	102.5	108.4	64.6	Sun obscured by dark clouds, 165° + 35°.
	10 35	90+84.5	5 29.8	9.3	117.7	127.4	66.0	Sun clear.
Sun's rays concentrated by lens: Needle beyond the focus of lens: two semicircles of blue glass over.	11 20	90+84	5 33.4	18.5	114.5	143.0	68.8	Sun clear.
	11 29	90+86	5 34.0	17.3	107.5	140.0	69.5	Sun obscured, 120 sec.
	11 40	90+86.5	5 34.3	18.5	107.4	147.0	69.8	Sun obscured, 15 sec.
	11 58	90+85.5	5 34.0	20.8	104.5	142.5	70.0	Sun clear.
	0 10	89.5+83	5 32.6	19.5	109.0	143.0	72.0	Sun clear.
Blue glass removed Lens edge- wise to the Sun Lens re- moved	0 22	90+84.8	5 34.4	10.5	128.0	141.0	73.0	Light haze with coloured halo round the sun.
	0 44	90+84	5 33.8	15.5	113.9	115.3	73.0	Sun scarcely seen; obscured by dense haze.
	0 58	90+84.8	5 34.2	17.5	111.0	108.2	74.0	Sun cast a very faint shadow.
Screened from the Sun.	1 11	90+84.8	5 34.3	20.5	104.6	122.4	74.6	Sun not clear; faint haze over.
	1 21	90+85.5	5 33.6	21.5	97.0	132.7	73.6	Sun clear at intervals.
	1 35	90+85.3	5 33.7	20.5	91.7	113.6	71.8	Sun hid entirely by a hazy cloud.
			Time of making 50 vibrations.					The intensity of the needle had been so much reduced by having the rays concentrated on it in some attempts with the lens, that the time of vibration was increased.
Exposed to the Sun.	3 15	90+85.3	5 34.1	16.0	101.3	127.2	74.0	Sun obscured by a light hazy cloud.
	3 24	90+85	5 34.7	13.0	111.5	128.7	74.0	Sun slightly obscured by very light haze.
	3 45	90+85	5 33.8	15.0	117.0	132.2	74.0	Sun slightly obscured by very light haze.
Screened from the Sun.	3 56	90+85	5 34.4	20.3	104.0	130.0	74.6	Sun obscured by light haze.
	4 07	90+84.5	5 33.4	21.0	93.5	122.0	74.0	Sun obscured by hazy cloud.
	5 43	90+84.5	5 34.4	22.5	79.5	80.5	72.2	Sun obscured by cloud.
	5 54	90+84.5	5 34.6	22.0	79.0	80.3	72.2	Sun obscured by cloud.

From these observations it appears, that the diminution in the terminal arc in all cases corresponded to the intensity with which the sun shone on the needle; that this arc was in all cases increased by a screen being interposed, whether the screen entirely excluded the direct rays, as when the wooden screen was interposed, or only partially, as in the cases of the sun being obscured by haze or cloud, or the needle being covered with blue glass, which

appeared to act simply as a screen; and that the diminution in the terminal arc did not correspond to the heat imparted to the needle or the medium in which it vibrated, excepting so far as this in some cases might correspond with the intensity of the sun's rays.

In the following observations I varied some of the circumstances: the day was most favourable for them, the sky continuing cloudless throughout, and the heat of the sun great, in the middle of the day intense.

Circumstances under which the Needle vibrated.	Time of commencing the Observation.	Extent of the first arc of Vibration.	Time of making 100 Vibrations.	Terminal Arc.	Thermometer.			Remarks.
					Within the Compass-box.	Exposed to the Sun.	In Shade.	
Screened from the Sun.	July 30th.							The sky cloudless throughout the day, and the heat of the sun very great, in the middle of the day intense.
	10 27	90+86.5	41.0	19.5	77.5	139.8	64.8	
	10 36	90+86.5	41.0	30.5	77.5	139.5	68.2	
Screened from the Sun.	10 45	90+86.3	42.2	20.5	78.4	143.5	69.0	{ The full effect does not appear to have been produced immediately.
	10 56	90+86.5	40.4	12.8	97.7	146.0	70.0	
	11 20	90+86.5	40.2	8.3	130.5	145.3	71.8	
Exposed to the Sun.	11 30	90+86.5	40.5	8.0	136.5	146.0	72.0	{ The full effect does not appear to have been produced immediately.
	11 44	90+86	41.6	17.5	124.5	147.0	72.3	
	11 56	90+86	42.6	21.0	101.0	149.0	73.3	
Screened from the Sun.	0 06	90+86	42.6	21.5	96.0	152.0	74.0	{ The full effect does not appear to have been produced immediately.
	0 23	90+87	41.7	12.5	114.0	150.4	74.2	
	0 33	90+86	41.7	13.0	120.3	151.8	74.6	
Exposed to the Sun, but screened by a glass over the Compass-box.	0 44	90+85.8	42.2	13.0	124.4	150.8	75.0	{ The full effect does not appear to have been produced immediately.
	1 11	90+85	42.4	13.0	129.0	150.7	75.4	
	1 26	90+85.5	42.7	12.8	132.9	149.0	76.0	
Two sections of red glass over the Needle.	1 36	90+85.5	42.6	7.3	141.0	148.5	76.0	{ N. end of the needle vibrates, on the west side, in the violet rays from the lens. { The violet rays (further from the N. end of the needle. { The violet rays again nearer to the N. end of the needle.
	1 48	90+85	41.9	5.8	156.0	150.2	77.0	
	1 59	90+85.3	41.4	6.8	156.0	150.4	77.3	
The semi-circles of red glass removed.	2 13	90+85.3	41.8	13.5	137.8	151.0	78.0	{ N. end of the needle vibrates, on the west side, in the violet rays from the lens. { The violet rays (further from the N. end of the needle. { The violet rays again nearer to the N. end of the needle.
	2 26	90+85.5	43.0	17.5	124.0	149.9	78.0	
	2 37	90+86	42.8	14.5	+119.5	148.0	78.2	
Two sections of red glass over the Needle.	2 48	90+86	42.6	15.0	+116.5	145.5	78.3	{ N. end of the needle vibrates, on the west side, in the violet rays from the lens. { The violet rays (further from the N. end of the needle. { The violet rays again nearer to the N. end of the needle.
	3 04	90+85.5	41.4	11.0	+126.5	140.0	78.4	
	3 15	90+84.5	41.4	13.3	+125.5	137.0	78.4	
Blue glass re-entrant to centre of compass-box.	3 33	90+86	42.6	5.5	165.5	135.0	78.4	{ Rays of greatest intensity on S. side of compass-box. { Rays of greatest intensity on centre of needle.
	3 43	90+85	43.4	3.8	167.0	130.5	78.2	
	3 53	90+84.8	41.8	7.0	158.0	129.0	78.2	
Left re-entrant needle exposed to the Sun.	4 04	90+85.8	42.6	8.8	139.5	123.0	78.4	{ The full effect does not appear to have been produced immediately. * This thermometer had now become shaded by a wall.
	4 14	90+85	42.8	17.5	128.5	120.0	78.0	
	5 55	90+86	44.2	21.5	82.0	*79.5	74.0	
Screened from the Sun.	6 05	90+85.8	43.8	21.5	81.0	*78.3	73.2	{ The full effect does not appear to have been produced immediately. * This thermometer had now become shaded by a wall.
	6 14	90+86	43.8	21.8	80.0	*76.4	73.0	

Although it appears from these observations, as from the preceding ones, that the diminution of the terminal arc did not in all cases correspond to the temperature of the needle or of the medium, yet it depended essentially on the intensity of the rays in the space in which the needle vibrated; the terminal arc being $3^{\circ}.8$ when the rays were most concentrated, and $5^{\circ}.5$, $5^{\circ}.8$, $6^{\circ}.8$, 7° , 8° , and $8^{\circ}.8$ as their intensity was diminished. The results obtained with the coloured glasses appear to indicate that the red rays had a greater effect in diminishing the terminal arc than the blue; for although the blue on the glass was very transparent, and the red far from being so, yet when the needle was screened by the blue glass, the terminal arc was $17^{\circ}.5$, and when screened by the red 13° , the thermometer within the compass-box indicating nearly the same temperature in the two cases. The effect which I observed on throwing the violet rays, as separated by the lens, further from the end of the needle and then bringing them nearer, although not very decided, favours the same conclusion. No further effect than that of partially screening, appears to have been produced by making one end of the needle vibrate in the blue rays while the other vibrated in the direct light of the sun.

If we may conclude from these observations, that the red rays are those which cause the diminution in the terminal arc, we might infer that the heat imparted to the needle or to the air was the cause of this diminution; but the experiments themselves show that this was not the case. However, to remove all doubt on this part of the subject, I determined to observe the terminal arcs with the different needles when their temperature and that of the medium in which they vibrated varied, and all other circumstances remained precisely the same.

For the purpose of making these observations, I placed a graduated circle of paper in a shallow vessel of earthenware, 7.5 inches in diameter, and 1.2 inch deep, having a rim projecting 1.2 inch all round. This rim rested on the rim of another vessel of earthenware 9.4 inches in diameter, and 3.6 inches deep, and which contained hot or cold water, according to circumstances, so as to surround the upper vessel to its rim. The glass cover of the compass, having in its centre the glass tube carrying the wire of suspension, was placed over the upper vessel, which thus supplied the place of a compass-box. The needles were successively suspended within this vessel by the same wires and in the same stirrups as before; the magnetized needle by the very fine brass wire; the glass, the copper, and the unmagnetized steel needle by the much thicker

wire, which caused them to vibrate in nearly the same time as the magnetized needle. So that, as before, the needles and suspending wires were the only substances of metal used in the apparatus. The upper vessel contained the small spirit thermometer which had before been placed in the compass-box. The ivory scale rested upon pieces of cork, which prevented the contact of the bulb or scale with the bottom or sides of the vessel; so that this thermometer indicated pretty accurately the temperature of the air within the vessel in which the needle vibrated; and likewise nearly that of the needle itself, by allowing some time to elapse, after making a considerable change in the temperature, previously to commencing the observations.

In making the observations, the under vessel was first filled with cold water, and the apparatus so adjusted that the needle pointed to zero on the graduated circle when the wire was devoid of torsion. The needle was then made to vibrate until the arc on the western side of zero was as near to 90° as I could obtain*; the time of its next passing zero was noted, and also the arc of vibration on the other side. At the hundredth vibration, the time and the arc beyond zero were noted, as before. The state of the thermometer in the upper vessel was taken at the commencement and likewise at the conclusion of the observation, the mean being considered as the temperature of the needle and of the air in which it vibrated. After thus making three observations, the upper vessel containing the needle was removed, the cold water contained in the lower one poured out, that vessel heated and filled with boiling water, and the upper vessel replaced and adjusted as before. Cloths steeped in boiling water were placed over the glass cover, for the double purpose of more rapidly heating the air in the vessel in which the needle vibrated, and of preventing the deposition of dew on the under surface by condensation. When the temperature of the air within the upper vessel had nearly attained its maximum, observations on the vibrations of the needle were made as before. The lower vessel was again filled with cold water, and the observations repeated. The following table contains two sets of observations with the magnetized steel needle: in the first set, the plane of the needle having been somewhat inclined laterally

* The magnetized needle was made to vibrate by means of a weak magnet held on the outside of the box, and the other needles by turning the index to which the upper part of the suspending wire was attached, and again bringing that index to zero. As I could have no perforation either in the side of the vessel containing the needle, or in the glass cover, I was obliged to adopt this method.

to the horizon, it was rendered horizontal previous to making the second, which is the reason that the terminal arcs in the first are rather less than the corresponding arcs in the second.

	Extent of 1st Vibration.	Time of making 100 Vibra- tions.	100th or Termi- nal Arc.	Inclosed Thermo- meter.	Termi- nal Ex- cess.	Difference of Tempe- ture.
Magnetized Needle. Weight = 197 grains.	90 + 88	5 55.0	28.5	62.0		
	90 + 88	5 55.4	28.75	62.5		
	90 + 88	5 55.1	28.5	63.0		
Means.....	90 + 88	5 55.2	28.6	62.5		
Means at High Temperature	90 + 89	6 01.8	34.5	137.5		
	89 + 87.5	6 02.2	34.0	139.0		
	90 + 89	6 00.8	33.75	137.5		
	89.7 + 88.3	6 01.6	34.1	138.0	5.05	75.1
Means at Low Temperature	90.5 + 88	5 54.8	29.5	64.25		
	90.5 + 88	5 54.7	29.5	62.5		
	90.5 + 88	5 54.75	29.5	63.37		
Means.....	90.25 + 88	5 55.0	29.05	62.9		
Magnetized Needle. Weight = 197 grains.	90 + 89	5 55.2	33.0	50.0		
	89.5 + 89	5 55.2	32.0	50.0		
	91 + 90	5 55.7	32.0	50.0		
Means.....	90.2 + 89.3	5 55.4	32.3	50.0		
Means at High Temperature	90 + 90	6 01.8	36.5	124.5		
	90 + 90.5	6 02.2	37.0	128.5		
	90 + 89	6 01.5	36.5	126.0		
	90 + 89.8	6 01.8	36.7	126.3	4.1	75.2
Means at Low Temperature	91 + 90.5	5 56.4	33.0	53.5		
	89.5 + 89	5 55.8	32.75	52.0		
	90.25 + 89.75	5 56.1	32.9	52.75		
Means.....	90.2 + 89.5	5 55.7	32.6	51.1		

Since, as might have been anticipated, it appears here that the effect upon the terminal arc, produced by an increase of temperature, is decidedly the reverse of that caused by the direct influence of the sun's rays, it follows that the latter effect does not arise from an increase in the temperature of the needle or of the medium in which it is vibrated. That there might be no doubt on the

subject, I repeated the observations with the magnetized needle, and made corresponding observations with the other three, the two steel needles having been previously rendered, as before, of the same weight as the others. In these observations, contained in the following table, the extent of the arc at the hundredth vibration was observed on both sides of zero, except with the copper needle.

Needle vibrated.	Extent of 1st Vibration.	Time of making 100 Vibrations.	Extent of 100th Vibration or Terminal Arc.	Inclosed Thermometer.	Terminal Excess.	Difference of Temperature.
Magnetized Steel Needle. Weight = 252½ grains.	90 + 88.5	5 58.8	33 + 33.5	51.5		
	90 + 88	5 58.5	33.5 + 33.5	51.0		
	90 + 88.5	5 58.2	33.5 + 33.5	51.5		
	Means.....	90 + 88.3	5 58.5	33.3 + 33.5	51.3	
		90 + 88	6 13.2	40 + 40	140.5	
		90 + 88	6 11.8	39 + 38.5	135.5	
		90 + 88	6 11.8	41 + 39.8	134.0	
	Means at High Temperature.	90 + 88	6 12.3	40 + 39.4	136.7	5.8
		90.5 + 88.5	6 06.5	34 + 34	51.75	
		90.5 + 88.5	6 05.0	35 + 34	51.75	
		90.5 + 88	6 05.2	35 + 34	51.75	
	Means.....	90.5 + 88.3	6 05.6	34.7 + 34	51.75	
Means at Low Temperature.	90.3 + 88.3	6 02.05	34 + 33.8	51.5		
Unmagnetized Steel Needle. Weight = 252½ grains.	90 + 87	5 52.8	21 + 22	49.0		
	90 + 88	5 52.6	21 + 23	49.5		
	90 + 87	5 52.6	21 + 23	50.0		
	Means.....	90 + 87.3	5 52.7	21 + 22.7	49.5	
		89 + 87	5 51.4	26 + 26.5	143.5	
		90 + 88	5 50.8	25.5 + 26.5	139.0	
		90 + 87.5	5 50.5	26 + 26.5	130.5	
	Means at High Temperature.	89.7 + 87.8	5 50.9	25.8 + 26.5	137.7	4.4
		90.5 + 88	5 50.0	22 + 22	48.0	
		90 + 87.5	5 49.9	21.5 + 22	48.5	
		90 + 87	5 49.8	21.5 + 21.5	49.5	
	Means.....	90.2 + 87.5	5 49.9	21.7 + 21.8	48.7	
Means at Low Temperature.	90.1 + 87.4	5 51.3	21.3 + 22.2	49.1		

Needle vibrated.	Extent of 1st Vibration.	Time of making 100 Vibrations.	Extent of 100th Vibration or Terminal Arc.	Inclosed Thermometer.	Terminal Excess.	Difference of Temperature.
Copper Needle. Weight = 252½ grains.	90 + 89	m s 5 51.1	° 28	57.0		
	89.5 + 88	5 51.2	28	57.0		
	90 + 88.5	5 51.2	28	57.0		
	Means.....	89.8 + 88.5	5 51.2	28	57.0	
	91 + 89	5 49.9	32	136.5		
	89.5 + 89	5 49.8	32.3	138.0		
	90 + 88.5	5 49.6	32	135.0		
	Means at High Temperature.	90.2 + 88.8	5 49.8	32.1	136.5	4.0
	90 + 89.5	5 51.2	28	57.5		
	90 + 88	5 51.0	28.5	56.0		
Glass Needle. Weight = 252½ grains.	90 + 88.5	5 51.2	28.5	56.0		
	Means.....	90 + 88.5	5 51.1	28.3	56.5	
	Means at Low Temperature.	89.9 + 88.5	5 51.15	28.1	56.75	
	90.5 + 90	5 56.0	27	51.0		
	90 + 88	5 56.2	26.5	51.0		
	90 + 88.5	5 56.2	26.5	52.0		
	Means.....	90.2 + 88.8	5 56.1	26.7	51.3	
	90 + 88	5 56.6	32.5 + 31.5	142.0		
	90 + 88.3	5 56.4	31 + 32	139.0		
	90 + 88	5 56.4	32 + 31.5	135.5		
Glass Needle. Weight = 252½ grains.	Means at High Temperature.	90 + 88.1	5 56.5	31.8 + 31.7	138.8	5.4
	90 + 88	5 56.4	26 + 25.5	57.0		
	90.5 + 88.5	5 56.0	26 + 26	51.5		
	91 + 90	5 56.2	26.5 + 26	50.75		
	Means.....	90.5 + 88.9	5 56.2	26.2 + 25.8	53.1	
	Means at Low Temperature.	90.3 + 88.8	5 56.2	26.3	52.2	

The effects here are not very different with the four needles. The glass needle and the two steel needles presenting broader surfaces on their sides to the air's resistance than the copper needle, it was to be expected that the terminal excess with these should be greater than with that needle; and I am not aware of any circumstance, except the shorter time during which the vibrations continued, that should have rendered it less with the unmagnetized

steel needle than with the other two. The principal fact, however, which I looked to ascertaining by these experiments was, whether the terminal excess was of the same character here when the temperature was increased, as that arising from vibrating the needle successively in the shade and exposed to the sun, when an increase of temperature likewise took place; and on this point they were quite conclusive, showing clearly, that if the terminal excess is considered plus in the latter case, it will be minus in the former. It is evident then from all these results, that if the rays of the sun had simply, uniformly increased the temperature of the medium in which the needle vibrated, and of the needle itself, the effect would have been in all cases to increase the length of the terminal arc, instead of diminishing it, as was invariably the case when exposure to the rays of the sun caused an increase of temperature. There can therefore be no doubt that the influence of the sun was not confined to uniformly heating the medium and the needle, but that, in all cases, other effects than would arise from this were produced, and that the influence upon the magnetized needle was very different from that upon either of the others.

I next proposed to determine the effects that would be produced on the arcs of vibration by the heat of a fire. For this purpose I placed the apparatus before a strong fire, at the distance of about two feet from the front, and six inches below the bottom of the iron grate containing it, and vibrated the magnetized needle, and likewise the glass needle, when successively screened from, and exposed to its direct influence. The observations, which are contained in the subjoined Table, show that the effect produced by exposure to the fire, though small, was to bring the needle sooner to rest, or that it was of the same character as that produced by exposure to the sun. I am not disposed to lay any stress upon the circumstance that the terminal excess was almost precisely the same in the two cases, but to attribute it to the small errors in the observations having accidentally so compensated each other as to produce this very close agreement. Indeed, if I could have detected such minute differences, the terminal excess ought to have been rather greater with the magnetized than with the glass needle, since the intensity of the fire's heat appeared to be greater with the former than with the latter. We may, however, infer from these experiments, that if the intensity of the fire's heat had been precisely the same with the two needles, no sensible difference would have been observed in the effects produced on them. The observations were

made in this order: The first three observations with the apparatus screened from the fire by a thick board; then immediately the four with the screen removed; after which it was replaced, and the other observations made.

Needle vibrated.	Needle vibrated screened from the Fire.				Needle vibrated exposed to the Fire.				Terminal excess.	Difference of Temp.
	Extent of 1st Vibration.	Time of making 100 Vibrations.	Extent of 1000 Vibrations or Terminal Arc.	Inclosed Therm.	Extent of 1st Vibration.	Time of making 100 Vibrations.	Extent of 1000 Vibrations or Terminal Arc.	Inclosed Therm.		
Magnetized Needle. Weight = 252½ grains.	90 + 89	^{m s} 5 48.2	35 + 34.5	51.0						
	90 + 88.5	5 48.4	34.5 + 34.3	51.5						
	90 + 89	5 48.4	34.5 + 34.3	52.0	90 + 89	^{m s} 5 48.8	34.3 + 34	58.5		
	90 + 89	5 49.2	34.5 + 34	84.0	90 + 89	5 49.8	33.5 + 34	76.0		
	90 + 88.5	5 48.8	34.5 + 34.3	76.5	90 + 88.5	5 50.4	33.5 + 34.2	83.0		
	90 + 89.5	5 48.7	35 + 34.5	67.0	90 + 89	5 50.6	33.8 + 34	90.0		
	Means...	90 + 88.9	5 48.6	34.7 + 34.3	63.7	90 + 88.9	5 49.9	33.8 + 34	76.4	^o 0.57
Glass Needle. Weight = 259½ grains.	90 + 84	5 56.8	25 + 25.5	58.0						
	90 + 85	5 57.2	25.2 + 25.8	58.25	90 + 84.5	5 58.0	24 + 25.5	62.5		
	90 + 84	5 57.3	25 + 25.5	58.75	90 + 85	5 58.4	24 + 25.3	70.0		
	90 + 84	5 57.8	25 + 25.3	74.5	90 + 85	5 58.6	24.2 + 25.3	75.0		
	90 + 84	5 57.5	25 + 25.3	71.5	90 + 85	5 58.6	24.2 + 25	77.0		
	90 + 84.2	5 57.3	25 + 25.5	64.25	90 + 84.9	5 58.4	24.1 + 25.8	71.1	0.56	6.9

From all the experiments which I have described, there can, I think, be no doubt that the rays of the sun have a peculiar influence on a magnetized needle, which causes, when such a needle is exposed to them, a greater diminution in its arcs of vibration than in those of any other needle under like circumstances; and that this effect is independent of the heat imparted to the needle or to the medium in which it vibrates. That part of the effect which is produced on all needles may perhaps be caused in this manner: as the air directly below the needle is in the shade, and therefore colder than that above it, a current of air will pass the edges of the needle, which may considerably increase the resistance and consequently diminish the terminal arc; and this may possibly account for the full effect not taking place immediately, since these currents would not be excited in full force immediately on exposing the needle to the sun, nor on screening it would they immediately subside*. Had this, however, been the

* If this is the cause of the terminal excess with the non-magnetic needles, this excess would nearly vanish if a wire frame of the same form as the needles were successively vibrated in the shade and exposed to the influence of the sun, since in this case there would be little shadow below the needle. This experiment I proposed making, but have not yet had an opportunity.

only cause of the terminal excess with the magnetized needle as well as with the others, this excess would have been nearly the same for all, or it would only have differed according to the thickness of the needle. The terminal excess ought in this case to have been nearly the same for the glass, the unmagnetized steel and the magnetized steel needles, and less for the copper than for either of these. This accords, in some measure, with the experimental results which I obtained with the copper needle, the glass needle, and the unmagnetized steel needle; but as the effect on the last was only about four-sevenths of that on the magnetized steel needle, we must, for part of the effect upon this needle, look to causes distinct from that which produced the effect on the others.

It is extremely difficult to point out any principle to which this effect upon the magnetized needle can be referred. Are we to infer from these experiments that light and magnetism have relative density, although the density of either is evanescent with regard to that of the rarest gas; and that therefore light may offer a sensible resistance to magnetic particles in their passage through it, and consequently also to the bodies with which they are united? That light is of such extreme tenuity as to offer no resistance to the passage of the rarest gas,—of which, however, we have no proof,—cannot be adduced as an argument against such an inference. Or, is it possible that the effect may be produced in a manner similar to that on needles vibrating within metallic rings? If this be the case, we must suppose that the rays become magnetic by induction, in their passage by the needle, and that their maximum of magnetism not being developed until after they have passed it, the most magnetic rays will always be in the rear of the needle, and by their attraction impede its progress. If the effect is produced in this manner, we might expect, when a strong magnet is brought near to a ray of light transmitted through a small opening into a dark room, that the ray would be inflected. I have not had an opportunity of making this experiment in a conclusive manner, but in the trials which I have made I have observed no such effect.

In the conclusion of my former paper I stated, that as magnetic influence in the compound solar rays was indicated by the effects which I had described, this would tend to remove the doubts which had been entertained respecting the results obtained by MORICHINI, by means of the violet rays; and Mrs. SOMERVILLE's paper, read almost immediately after mine, describing the effects

which that lady had observed to be produced under different circumstances by the more refrangible rays, appeared completely to verify MORICINI's results, and to corroborate my opinion. Although the experiments of Mrs. SOMERVILLE have, on repetition, in many instances failed, we cannot, seeing the precautions that were taken, suppose that the effects described were due to other causes than the influence of the rays, but must rather infer that we are not aware of all the circumstances which may interfere with the success of the experiment. It cannot, however, be denied that the subject is at present involved in much mystery; and it is therefore very desirable that the circumstances on which the success of Mrs. SOMERVILLE's experiment depends should be clearly ascertained, and that the effects which I have invariably found to be produced by the compound rays should be traced to some known principle of action. I had proposed to myself to make several series of observations with the view of obtaining comparative results with different azimuths of the sun, at different seasons of the year, and likewise with the horizontal needle and the dipping needle vibrated both in the meridian and at right angles to it; and also to determine, with some degree of precision, the effects produced by the several separated rays under different circumstances. But in the experiments which I have already made, I have met with so many and such vexatious interruptions, arising principally from the uncertainty of our climate, and partly from my not always being able to avail myself of a favourable state of the weather, that seeing no prospect of succeeding in experiments requiring continued clear weather and uninterrupted leisure, I must leave them to be made by those who may be placed under more favourable circumstances, and be content to prosecute the inquiry by making such experiments as intervals of leisure, which I may have during fine weather, will allow.

*Royal Military Academy,
2nd June, 1828.*

S. H. CHRISTIE.

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TO THE

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METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

KEPT BY THE ASSISTANT SECRETARY

AT THE REQUEST OF THE

ROYAL SOCIETY

OF LONDON

THE PRESIDENT AND COUNCIL

1828. January.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point 9 A.M. in de- grees of Fahr.	External Thermometer.				Rain, in Inches, Based on at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		Fahrenheit.		Self-registering.				
						9 A.M.	3 P.M.	Lower.	Higher.			
1	29.531	44.7	29.379	45.8	45	45.8	46.6	38.2	46.6	0.044	SE	Overcast and foggy. Showery.
2	29.893	43.0	29.740	43.7	38	37.7	43.2	36.0	49.2	0.522	WSW	Overcast. Showery.
3	29.439	46.7	29.582	47.7	45	45.4	43.8	37.2	46.8	0.128	WSW	Fine and cloudless. Showery.
4	29.853	42.3	29.656	44.8	37	37.1	39.6	34.3	42.3		WSW	{ A.M. Fine—light clouds. P.M. Heavy rain—brisk wind.
5	29.625	40.7	29.615	41.3	35	35.0	35.7	32.6	35.8	0.283	SW	Cloudy—continued snow, with rain.
6	29.702	40.2	29.796	40.7	36	36.3	37.0	34.4	37.7	0.261	ESE	Lightly overcast—foggy.
7	29.963	39.3	29.923	39.3	34	35.3	35.0	32.6	35.3		ESE	Overcast and hazy.
8	29.968	37.3	29.899	38.7	27	34.3	36.8	32.7	36.8		E	Overcast and hazy.
9	30.004	36.7	29.950	36.6	30	33.2	32.1	32.1	32.3		NNE	Hazy. A.M. Overcast. P.M. Cloudless.
10	29.710	34.6	29.628	36.2	27	30.2	31.9	28.2	31.9		SSW	{ Overcast and foggy. Light fall of snow at night.
11	29.523	34.2	29.525	36.2	30	30.6	34.9	28.0	40.7		N var.	Overcast. Showery.
12	29.763	37.6	29.719	39.3	37	37.6	42.7	29.8	46.2	0.230	SW	{ Strong fog. Violent wind, with rain in the night.
13	29.149	43.4	29.378	44.4	47	46.6	45.8	36.8	46.6	0.569	SSW	Lowering. Strong wind, with rain.
14	29.564	43.3	29.543	44.3	41	40.7	41.3	39.8	41.8	0.069	SW	Overcast and foggy. Evening showery.
15	29.721	41.3	29.790	41.7	36	36.4	36.2	35.2	36.4	0.242	N	Overcast & hazy. Snow in the evening.
16	29.547	39.3	29.806	40.2	36	36.3	38.2	32.7	43.2	0.472	E	Overcast and foggy.
17	29.922	41.2	29.944	43.8	44	44.0	47.8	35.4	50.0	0.208	ESE	Overcast and foggy. Rain.
18	30.124	47.8	30.177	51.0	51	50.6	53.7	43.3	54.5	0.128	S	Overcast and foggy.
19	30.263	49.8	30.244	52.8	46	47.7	52.8	45.8	53.8	0.005	SSW	Fine and cloudless—hazy.
20	30.303	51.3	30.295	51.9	50	50.3	51.2	46.4	51.2		SW	{ A.M. Cloudy. P.M. Fine and nearly cloudless.
21	30.274	48.8	30.235	50.8	43	43.0	49.2	41.7	49.3		SW	{ A.M. Lightly overcast. P.M. Clear and cloudless.
22	30.113	50.0	30.149	52.5	49	49.2	52.7	42.3	53.4		SSW	Fine—light clouds.
23	30.267	49.4	30.282	51.8	47	46.8	51.2	43.2	51.2		WSW	{ A.M. Dull—dissolving clouds. P.M. Clear and cloudless.
24	30.367	49.4	30.334	51.7	46	46.6	50.8	43.3	50.8		SW	Overcast and hazy.
25	30.306	51.8	30.127	52.8	50	49.7	51.2	46.1	51.5		S	Lowering—light haze.
26	30.261	49.2	30.231	51.3	43	42.7	48.3	41.2	48.3	0.018	W	Fine and cloudless—light haze.
27	30.398	48.6	30.409	51.6	44	44.4	50.0	41.9	50.0		WSW	{ A.M. Fine and nearly cloudless. P.M. Dull—hazy.
28	30.432	45.8	30.374	47.1	39	39.2	41.1	36.7	42.8		WSW	Overcast and foggy.
29	30.215	46.6	30.174	48.0	43	43.7	45.2	38.5	45.3		S	A.M. Foggy. P.M. Fine and cloudless.
30	30.100	45.7	30.055	47.7	43	42.7	45.3	38.8	45.3		SSW	Fine—hazy.
31	30.002	45.7	29.968	47.8	43	42.8	47.8	38.2	49.0		SSE	Overcast. Rain.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.939	44.1	29.933	45.6	40.7	41.4	43.8	37.5	45.0	3.159		
Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr.											{ 9 A.M. 29.912	{ 3 P.M. 29.902 }
OBSERVANDA.												
Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge..... = 83 feet 2½ in.												
..... above the mean level of the Sea (presumed about) = 95 feet.												
The External Thermometer is 2 feet higher than the Barometer Cistern.												
Height of the Receiver of the Rain Gauge above the Court of Somerset House = 79 feet 0 in.												
The hours of observation are of Mean Time, the day beginning at Midnight.												
The Thermometers are graduated by Fahrenheit's Scale.												
The Barometer is divided into inches and decimals.												

METEOROLOGICAL JOURNAL FOR FEBRUARY, 1828.

1828. February.	9 o'clock, A.M.		5 o'clock, P.M.		Dew Point at 9 A.M. in de- grees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
	Barom.	Atmos. Therm.	Barom.	Atmos. Therm.		Fahrenheit.		Self-registering.				
						9 A.M.	3 P.M.	Lowest.	Highest.			
○ 1	29.942	49.8	30.057	53.3	49	49.8	50.8	43.4	52.2	0.139	SSW	{ A.M. Lowering and showery. P.M. Fine—lightly overcast.
h 2	30.124	49.9	30.103	51.2	47	46.8	48.0	45.2	48.0		SW	Lightly overcast—bary.
○ 3	30.439	46.8	30.477	48.3	37	39.2	45.2	37.7	46.7		W	Fine and cloudless—bary.
h 4	30.250	47.3	30.263	49.9	47	47.4	51.3	38.3	51.2	0.019	WSW	Lightly overcast—bary.
h 5	30.192	49.7	30.140	51.6	48	48.6	51.3	46.8	51.3		SW	Lowering.
h 6	30.054	50.6	30.010	52.3	48	48.7	52.8	47.2	52.8	0.012	SW	Cloudy.
h 7	29.944	50.4	29.901	52.6	45	45.2	49.2	44.5	49.8		SW	Foggy—rain.
h 8	29.911	49.3	29.831	47.3	41	42.3	45.3	41.2	45.8	0.097	N	{ A.M. Cloudless. P.M. Lightly over- cast.
h 9	29.660	45.8	29.681	46.2	41	45.4	40.3	39.3	40.3	0.039	N	Overcast—rain.
○ 10	29.812	42.7	29.772	43.6	36	36.2	36.9	34.4	36.9	0.059	N	Lowering.
h 11	29.454	38.2	29.602	40.6	32	33.2	33.2	30.2	34.2		NNE	Continued fall of snow.
h 12	29.555	35.8	29.593	37.2	30	30.8	31.7	29.0	31.7	0.186	NNW	Overcast. Fall of snow A.M.
h 13	30.115	35.0	30.098	36.3	30	30.8	32.3	28.8	32.3		NNE	Overcast and bary.
h 14	29.723	36.8	29.489	37.6	31	33.4	34.8	30.0	34.8		SE	Overcast. Fall of snow P.M.
h 15	29.667	38.2	29.742	41.1	34	34.7	39.4	31.2	39.4		W	Fine and cloudless—bary.
h 16	29.872	38.0	29.836	39.8	32	32.6	38.0	29.8	38.0	0.086	WSW	Overcast—Strong haze A.M.
○ 17	29.721	37.8	29.632	40.6	32	33.8	39.2	30.2	39.2		SE	{ A.M. Cloudless. P.M. Lightly over- cast.
h 18	29.294	37.4	29.297	42.0	34	34.6	42.2	30.8	42.2		E	Dull—light clouds.
h 19	29.203	39.7	29.233	43.6	36	38.7	45.9	33.8	45.9	0.078	WSW	Fine—light clouds.
h 20	29.277	41.8	29.202	45.3	41	42.8	47.4	37.3	47.7		SSE	{ Fine and clear—light clouds. Snow in the evening.
h 21	29.017	43.6	29.971	47.0	40	40.2	46.8	39.2	46.8	0.011	E	Lowering.
h 22	29.004	46.3	29.044	48.7	43	42.8	45.3	38.8	47.5	0.053	SE	Lowering.
h 23	29.292	46.1	29.418	46.7	43	42.7	41.8	41.3	42.7		N	Overcast and foggy. Light rain A.M.
○ 24	29.771	44.8	29.797	47.3	41	41.6	46.3	37.7	49.7	0.033	WSW	{ A.M. Overcast. P.M. Fine—light clouds.
h 25	29.857	48.1	29.867	50.2	49	49.8	51.6	41.0	52.0		S	Lowering—light breeze—showery.
h 26	30.029	51.3	30.012	53.3	51	52.2	55.0	48.8	55.2	0.090	S	Lowering.
h 27	30.115	52.3	30.144	54.3	51	51.3	53.2	49.1	53.7		SSE	Overcast.
h 28	30.322	52.3	30.329	54.3	49	48.9	50.3	47.2	50.7		N	Overcast and foggy.
h 29	30.340	52.7	30.357	54.8	50	50.3	53.6	41.7	53.6		NNW	Cloudy—light breeze.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.809	44.8	29.806	46.7	40.9	41.9	44.8	38.4	45.2	0.902		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. { 9 A.M. 3 P.M. }
 { 29.789 29.769 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge = 83 feet 2½ in.
 above the mean level of the Sea (presumed about) = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House = 79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

1898. March.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in de- grees of Fahr.	External Thermometer.				Rain, in inches Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.	
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		Fahrenheit.		Self-registering.					
						9 A.M.	3 P.M.	Lowest.	Highest.				
○ 1	30.290	50.0	30.175	50.7	44	44.2	47.9	41.1	47.9		N	Overcast—light fog.	
○ 2	30.261	50.3	30.057	51.3	45	46.5	48.2	43.7	48.2		NNE	Cloudy—light wind.	
○ 3	30.006	47.7	99.897	49.6	43	42.8	47.8	37.7	47.8		SSW	Cloudy and hazy.	
○ 4	29.974	47.8	99.858	52.0	44	43.9	49.2	41.3	48.6	0.007	NW	Cloudy—light fog.	
○ 5	29.689	47.8	99.682	45.1	39	42.3	42.0	39.7	43.5		NW	{ Brisk wind. A.M. Fine and nearly cloudless. P.M. Cloudy.	
○ 6	29.900	40.0	99.978	48.6	29	33.9	38.8	29.1	38.8		N	Fine—cloudy—light wind.	
○ 7	30.235	40.2	30.174	44.3	27	33.7	41.8	29.8	46.6		SW	A.M. Fine—hazy. P.M. Overcast.	
○ 8	30.082	44.3	30.151	48.3	46	46.8	54.7	32.8	55.5	0.111	WSW	Overcast and foggy.	
○ 9	30.262	48.3	30.210	51.0	48	48.2	55.8	46.7	56.5		WSW	{ A.M. Overcast. P.M. Fine—light clouds.	
○ 10	30.242	49.8	30.207	53.3	48	49.2	56.9	43.8	57.2		WNW	{ A.M. Lowering. P.M. Fine—light clouds.	
○ 11	30.260	49.6	30.226	53.2	46	46.0	54.0	41.8	54.3		SW	{ A.M. Overcast and foggy. P.M. Fine —light clouds.	
○ 12	30.078	50.7	30.025	53.6	48	48.2	53.8	44.0	53.8		S	A.M. Overcast. P.M. Fine—hazy.	
○ 13	30.135	52.6	30.131	55.8	50	50.3	55.5	46.8	60.6		SW	Clear and cloudless.	
○ 14	30.284	56.0	30.276	58.6	53	54.0	59.3	50.1	59.4		SW	Fine and cloudless.	
○ 15	30.268	54.3	30.258	58.6	48	48.1	59.6	43.8	60.2		SW	Fine—light clouds.	
○ 16	30.346	55.8	30.248	59.6	52	52.7	60.8	47.2	61.3		WSW	Fine—cloudy. Showery at night.	
○ 17	30.169	56.3	30.140	54.3	53	53.4	56.6	50.4	61.3	0.036	NW	Lowering.	
○ 18	30.074	53.5	30.053	57.7	50	50.6	55.8	47.7	55.8		NW	Fine—lightly overcast.	
○ 19	29.462	54.0	29.522	55.3	43	50.3	51.7	46.9	52.4		WSW	Fine—light clouds—strong brisk wind.	
○ 20	29.439	50.7	29.262	54.3	45	46.8	48.7	40.1	51.2		WSW	Lowering. Rain.	
○ 21	29.065	51.7	28.998	53.6	46	46.2	48.7	41.7	51.1	0.053	SSW	{ Cloudy. At 3 P.M. Hall storm, with thunder.	
○ 22	29.294	49.3	29.281	51.2	34	43.5	47.9	37.0	47.9	0.053	WSW	Fine and clear—cloudy. Showery P.M.	
○ 23	29.419	46.6	29.449	49.3	36	40.5	46.8	34.2	47.6	0.086	WSW	Fine, and nearly cloudless.	
○ 24	29.655	45.8	29.685	48.3	36	41.8	46.3	35.6	46.3		N	Fine—cloudy.	
○ 25	29.806	44.6	29.808	47.3	35	43.3	42.3	35.7	44.3		NW	{ Lowering—brisk wind. Rain and hail P.M.	
○ 26	29.861	41.3	29.791	45.6	35	35.7	44.3	31.7	44.8	0.080	WSW	Fine—light clouds—hazy.	
○ 27	29.465	44.4	29.430	49.3	43	43.8	51.2	34.7	52.5	0.025	ESE	Lowering. Showery.	
○ 28	29.476	46.7	29.594	47.5	42	41.7	43.4	38.8	43.4	0.208	N	Overcast—brisk wind. Rain.	
○ 29	29.413	44.3	29.822	47.3	39	39.2	42.8	34.5	45.3	0.002	WSW	Cloudy and hazy.	
○ 30	29.988	45.0	30.040	47.3	41	41.7	45.7	36.4	45.7		N	Lowering.	
○ 31	30.280	43.7	30.292	47.3	41	41.7	47.8	33.8	49.4		E	Fine—light clouds—hazy.	
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum			
	29.919	48.5	29.399	51.2	42.6	44.8	50.0	40.0	51.0	0.751			
Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr.										{ 9 A.M. 29.881 }		{ 3 P.M. 29.854 }	
OBSERVANDA.													
Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge.....										= 83 feet 2½ in.			
.....above the mean level of the Sea (presumed about)										= 95 feet.			
The External Thermometer is 2 feet higher than the Barometer Cistern.													
Height of the Receiver of the Rain Gauge above the Court of Somerset House.....										= 79 feet 0 in.			
The hours of observation are of Mean Time, the day beginning at Midnight.													
The Thermometers are graduated by Fahrenheit's Scale.													
The Barometer is divided into inches and decimals.													

1898. May.	9 o'clock, A.M.		3 o'clock P.M.		Dew-Point at 9 A.M. in degree Fahr.	External Thermometer.				Rain, in inches, in the Rain of 9 A.M.	Direction of Wind at 9 A.M.	Remarks.
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		Fahrenheit.		Self-registering.				
						9 A.M.	3 P.M.	Lowest.	Highest.			
1	30.384	58.8	30.514	62.1	59	52.4	61.7	46.8	62.7		E	{ A.M. Strong haze. P.M. Fine and clear—nearly cloudless.
2	30.390	57.3	30.182	62.0	51	51.4	57.8	44.3	60.4		ESE	Fine—light clouds.
3	30.017	56.5	29.994	60.3	53	53.6	56.4	48.2	58.5	0.014	ESE	Lowering. Rain.
4	29.788	59.4	29.719	61.7	55	56.8	60.8	51.6	63.3	0.042	S	{ Fine and clear.—A.M. Heavy shower. P.M. Distant thunder.
5	29.707	54.7	29.683	58.8	49	54.9	56.8	45.8	57.8	0.081	WNW	Fine—cloudy.
6	29.748	60.7	29.742	59.3	45	56.3	57.7	43.8	60.8		SSW	{ A.M. Fine and cloudless. P.M. Thunder and lightning at 3 1/2.
7	29.849	54.9	29.851	59.3	49	50.5	58.6	45.4	59.3		N	A.M. Overcast. P.M. Fine—cloudy.
8	29.883	54.3	29.901	59.0	49	48.9	58.3	46.2	59.5	0.186	N	{ A.M. Overcast. P.M. Fine and clear—gentle wind.
9	30.070	55.9	30.114	59.6	46	51.5	59.4	42.7	60.5		W	Fine—lightly overcast.
10	30.179	59.6	30.136	62.0	45	58.0	61.6	45.0	64.8		E	{ A.M. Fine and cloudless. P.M. slightly lowering.
11	30.085	59.4	30.060	61.8	55	57.5	62.8	51.2	63.8		WSW	{ A.M. Lowering. P.M. Fine—light clouds.
12	30.238	61.3	30.257	62.6	46	58.7	63.3	48.2	64.7		N	{ Fine & clear—nearly cloudless—light wind.
13	30.316	63.4	30.240	62.3	48	58.3	64.0	46.7	64.8		SSW	Fine and cloudless—light haze.
14	30.192	60.3	30.127	63.6	56	58.6	66.4	50.0	68.0		NNE	Fine and clear—fancy clouds.
15	30.070	61.8	30.029	64.3	51	60.7	66.2	47.0	66.4		E	Fine—light stretching clouds.
16	29.883	63.8	29.860	67.3	59	64.3	67.3	56.0	69.5		NE	{ Fine—light clouds. Thunder at 7 1/2.
17	29.832	66.8	29.811	66.7	54	63.2	66.8	55.9	67.3		E	Fine and cloudless—light wind.
18	29.816	63.7	29.826	64.5	51	61.7	61.9	55.0	63.7		E	Clear and cloudless—light wind.
19	29.832	64.6	29.829	63.2	46	62.2	62.7	46.8	63.7		ESE	Clear and cloudless—light wind.
20	29.903	61.4	29.760	63.2	50	59.4	60.7	45.0	66.2		E	A.M. Fine. P.M. Cloudy.
21	29.856	57.7	29.555	62.8	54	55.8	61.7	50.0	63.1		ENE	Cloudy. Light showers.
22	29.574	55.7	29.569	60.3	52	53.4	58.7	51.0	59.5	0.036	E	Lowering. Light showers.
23	29.611	60.3	29.593	63.0	49	57.7	62.1	49.8	62.6	0.014	NNE	{ Fine—cloudy—light wind.
24	29.481	61.3	29.422	65.0	54	58.8	63.8	55.1	66.6	0.294	S	{ A.M. Heavy rain, early. Fine & clear. P.M. Lowering, strong wind—showery.
25	29.074	63.7	29.813	64.3	55	61.7	64.0	55.6	65.4	0.041	SW	{ Fine and clear—light clouds—brisk wind.
26	29.756	63.3	29.635	65.3	54	62.5	65.0	53.6	66.8		ESE	Cloudy—light wind.
27	29.601	65.3	29.552	65.8	53	64.5	61.8	52.9	68.6		ESE	{ A.M. Fine and clear. P.M. Cloudy—brisk wind. Rain.
28	29.554	62.8	29.649	68.8	54	59.3	61.8	53.3	64.3	0.353	SW	Cloudy—light wind.
29	29.792	63.9	29.704	65.4	56	62.3	65.3	55.3	67.2	0.033	S	Fine—brisk wind. Showery.
30	29.950	67.4	29.959	68.2	53	65.3	70.4	55.3	72.1		N	Fine—cloudy. Rain at night.
31	29.840	63.8	29.792	67.2	60	60.5	67.1	56.1	68.4	0.213	SSW	{ A.M. Lowering—light wind. P.M. Fine—cloudy.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.890	60.7	29.860	63.0	51.7	58.1	62.3	50.0	64.2	1.307		
Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr.											{ 9 A.M. 29.820	{ 3 P.M. 29.784 }
OBSERVANDA.												
Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge											—83 feet 2 1/2 in.	
..... above the mean level of the Sea (presumed about)											—95 feet.	
The External Thermometer is 2 feet higher than the Barometer Cistern.												
Height of the Receiver of the Rain Gauge above the Court of Somerset House											—79 feet 0 in.	
The hours of observation are of Mean Time, the day beginning at Midnight.												
The Thermometers are graduated by Fahrenheit's Scale.												
The Barometer is divided into inches and decimals.												

